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Analytical and Critical Reviews.

I.—The Works of Sir J. Y. Simpson, Bart. Vol. II.[1]

THE papers collected in this second volume of Sir James Simpson's works are somewhat heterogeneous, and their common authorship would seem to be almost the only connecting link between them. They have all, with the possible exception of a few lecture-notes, been published before; those on Hospitalism, and the Stamping out of Contagious Disease, so recently as to be familiar to most of our readers. The elaborate essay on Hermaphroditism is a reprint of the well-known article in the "Cyclopædia of Anatomy and Physiology," published in 1839. The remaining subject of Anæsthesia was one in which Sir James Simpson took a peculiar interest, and with which his name will always be honourably connected. At the present day, when means are being rapidly multiplied for the alleviation of human suffering, it will not be uninteresting or uninstructive to be taken back some five-and-twenty years, and to read of the opposition offered to the introduction of Anæsthetics, and of the prejudice against their employment which existed even in the most enlightened ranks of the profession. It is much to be regretted that Sir James Simpson was not spared that he might himself have condensed the scattered facts on this subject into a continuous narrative; as it is, we can but sympathise with the editor in the difficulties of dealing with a mass of papers addressed to different persons or journals on the same subject, in which the same argument occurs over and over again, with a tiresome iteration, often the same words with a slightly different setting; some, as sharp retorts despatched across the Atlantic in reply to the angry and not over-courteous remarks of Dr. Bigelow; some, addressed from a sick bed to

[1] *Anæsthesia, Hospitalism, Hermaphroditism, and a proposal to stamp out Smallpox and other Contagious Diseases.* Edited by Sir W. G. SIMPSON, Bart., Scholar of Gonville and Caius Coll., Cambridge.

the *Edinburgh Monthly*, or the *Lancet;* others, mere lecture-notes, the dry bones which their author could so readily make live in the fervour of his popular discourses.

In many of the controversial papers, especially those on Hospitalism, we labour under the disadvantage of having one aspect only of the subject presented to us; no opinions could therefore be passed upon the points at issue without such an examination of all the arguments as would be impossible in the present notice. We shall content ourselves by giving a short *résumé* of the three subjects—anæsthesia, hospitalism, and stamping out disease—from the author's point of view, and referring those who take a more special interest in them to the volume itself, which although made up of chips, as it were, from a great workshop, representing disconnected thoughts rather than mature opinions on some of the great medical and sanitary problems of the day, is undoubtedly of very great interest, and of no mean practical value.

I. *Anæsthesia.*

The ancient history of anæsthesia, or nodynia (νωδυνια), as Theocritus ('Idyll,' 18) perhaps more properly calls it, is vague and unsatisfactory, notwithstanding that allusions are frequent to drugs possessing the power of alleviating pain. Indian hemp was used as early as the third century among the Chinese and in the East generally. The Mandragora officinalis (the basis of the spongia somnifera in the thirteenth century) is extolled in the highest terms by Dioscorides (iv, 76):—

"There is prepared also, besides the decoction, a wine from the bark of the root, three minæ being thrown into a cask of sweet wine; and of this three cyathi are given to those who are to be cut or cauterized, as aforesaid; for being thrown into a deep sleep, they do not perceive pain."

With the commencement of the present century we reach the second or modern stage of anæsthetics in the suggestion of Sir Humphry Davy, that,

"As nitrous oxide in its extensive operation appears capable of destroying physical pain, it may probably be used with advantage during surgical operations in which no great effusion of blood takes place."—('Chemical and Philosophical Experiments,' p. 464.)

Little attention, however, seems to have been paid to these discoveries until nearly half a century later, when the scene is shifted to America, and we there see the earliest operations performed under the influence of sulphuric ether and nitrous oxide gas. The dates and order of sequence in these discoveries, long the subject of dispute, are given by Sir James Simpson as follows:—

(1) On December, 11th, 1844, Dr. Wells had at Hertford, Connecticut, one of his molar teeth extracted without pain, after having deeply breathed nitrous oxide gas. The idea had been suggested to him by seeing Mr. (now Colonel) Cooley at a lecture on the previous evening wound his limbs severely against the benches while under the influence of the gas, without feeling any suffering from these injuries.

(2) On the 30th September, 1846, Dr. Morton, a former pupil and partner of Dr. Wells, extracted a tooth without pain whilst the patient was breathing sulphuric ether—this fact making a new era in anæsthetics and in surgery.

(3) On January 19th, 1847, the first case of midwifery in which sulphuric ether was adopted as an anæsthetic occurred at Edinburgh, under Sir James Simpson's care, before any case was tried in Boston or America.

(4) On November 15th, 1847, the anæsthetic effects of chloroform were discovered in Edinburgh, and this drug soon superseded all rivals in Scottish and English practice.

The opposition raised in this country to the use of chloroform, not only in midwifery, but in every form of surgical operation, can only be compared to that which had assailed the introduction of vaccination some forty years before. Childish and absurd as the arguments appear now, they had a real existence at the time, and had to be met vigorously by those who strove for the advance of truth and knowledge. Foremost among these was Sir James Simpson, and more than half of the volume we are reviewing is devoted to the defence of anæsthetics against their persevering and powerful detractors.

The objections raised to the practice of anæsthesia may be conveniently divided into those resting on moral, and those resting on physiological grounds. Some of these, perhaps most of them, have been consigned to the limbo of past experiences, dead faiths, and obsolete superstitions; others still hold their ground as strongly now as then, and sometimes thwart the physician in his endeavours to mitigate suffering or defeat disease. We suppose there are few mothers even now in the middle classes in England and in the more educated of the lower classes who would not quote the words of the original curse in Genesis (iii. 16, 17) against the exhibition of chloroform during labour. Yet the Hebrew text, if rightly translated, would seem to bear a very different meaning to that offered in our English Bibles:—

"16. Unto the woman he said, I will greatly multiply thy sorrow ('itztzabhon) and thy conception; in sorrow ('etzebh) thou shalt bring forth children; and thy desire shall be to thy husband, and he shall rule over thee.

"17. And unto Adam he said, Because thou hast hearkened unto the voice of thy wife and hast eaten of the tree, of which I commanded thee, saying, Thou shalt not eat of it: cursed is the ground for thy sake; in sorrow ('itztzabhon) shalt thou eat of it all the days of thy life."

On this Sir James Simpson remarks:—

"The state of anæsthesia does not withdraw or abolish that muscular effort, toil or labour; for if so it would then stop and arrest entirely the act of parturition itself. But it removes the physical pain and agony otherwise attendant on these muscular contractions and efforts. It leaves the labour itself ('etzebh) entire. And in relation to the idea that the Hebrew noun in the text truly signifies muscular *toil* and effort, and not physical *pain* and maternal agony, it is further highly important to remark that in the very next verse (17), viz., in the first part of the curse on man—the analogous Hebrew noun ('itztzabhon) which we translate by "sorrow," does *not* in any degree mean or imply mortal suffering or pain, but toil and labour. 'In sorrow shalt thou eat of it (the ground) all the days of thy life' (p. 47).

If, on religious grounds, your obstetric friends object to relieving a woman entirely of her worst pains, now that they have the means of doing so, they must, on the very same grounds, refuse to relieve her imperfectly and partially of these or any other pains and sorrows connected with parturition; they must, or, at least, ought to abstain, in fact, from all obstetric practices whatsoever; they should, in short, give up their present profession as a profession of sin—and 'in the sweat of their face' eat bread" (p. 59).

Another form of objection was that the use of anæsthetics must be irreligious, for it is so very "unnatural:"—

"I do not," writes an esteemed teacher of midwifery from Dublin, "believe that any one in Dublin has as yet used ether in midwifery; the feeling is very strong against its use in ordinary cases, and merely to avert the ordinary amount of pain which the Almighty has seen fit—and most wisely we cannot doubt—to allot to natural labour; and in this feeling I heartily and entirely concur."

Sir James Simpson playfully applies a like line of argument to the *first* introduction of carriages into use, and thus shows its absurdity:—

"I do not believe that any one in Dublin has as yet used a carriage in locomotion. The feeling here is very strong against its use in ordinary progression, and merely to avert the ordinary amount of fatigue which the Almighty has seen fit—and most wisely we cannot doubt—to allot to natural walking; and in this feeling I heartily and entirely concur."

In reading Part II, which is concerned with the defence of anæsthesia, it is difficult to realise that we are dealing with arguments used so late as 1847, though it affords another proof how

fast the world is advancing, and how different the thoughts of to-day are from those of even twenty years ago:—

"At a meeting of the South London Medical Society held in April last (1847) Dr. Gull read a paper on the injurious effects of ether inhalation, and ended his communication with queries as to 'the *desirability* of removing pain,' &c. Mr. Bransby Cooper, Surgeon to Guy's Hospital, afterwards affirmed it as his opinion that pain was a premonitory condition no doubt fitting parts, the subject of lesion, to reparatory action, and therefore he (Mr. Cooper) should feel averse to the prevention of it" (p. 81).

Thanks to the efforts of Sir James Simpson and his fellow-workers, the terrors of operating have almost ceased to exist, and the worst agonies of human suffering have been banished for ever. So little indeed do we of the present generation know of the mental and physical suffering with which our fathers were so familiar, that the eloquent letter from a distinguished surgeon, himself the subject of operation before the days of anæsthetics, will not be without interest at the present time. We have only space for one or two short quotations:—

"Suffering so great as I underwent cannot be expressed in words, and thus fortunately cannot be recalled. The particular pangs are now forgotten; but the black whirlwind of emotion, the horror of great darkness and the sense of desertion by God and man, bordering close upon despair, which swept through my mind and overwhelmed my heart, I can never forget, however gladly I would do so.

"From all this anguish I should of course have been saved had I been rendered insensible by ether or chloroform or otherwise before submitting to the operation. . . . When I made up my mind to submit to the operation proposed to me, it was with the fullest conviction that the pain it would occasion would far exceed my power of patient tolerance, and I prepared for it simply as for a dreadful necessity from which there was no escape. I awoke each morning from troubled sleep to reconsider the whole reasons for and against submitting to the surgeons, and by a painful effort reached again the determination not to draw back from my first resolution. From all this distracting mental struggle, which reacted very injuriously on my bodily constitution, I should have been exempted had I been able to look forward to the administration of chloroform. . . . The sum you will perceive of what I have been urging is that the *unconsciousness* of the patient secured by anæsthetics is scarcely less important than the *painlessness* with which they permit injuries to be inflicted on him" (p. 86 *et seq.*).

To meet objections of a different type, and from a medical point of view, more formidable than those we have already noticed, Sir James Simpson instituted an elaborate inquiry into the result of operations before and after the introduction of anæsthetics—an inquiry which proved, so far as limited figures

could prove, that the risks to life had been reduced; that "the fatality," for instance, following amputation of the thigh was not greater than one in every four operated upon when the patients were previously anæsthetised. It was as high as one in every two or three operated upon when the patients were not previously anæsthetised" (p. 105). And again, of labour, he says:—

"In cancelling the pains of parturition by anæsthesia we also, I believe, to a great extent cancel the perils of the process; for the mortality accompanying labour is regulated principally by the previous length and degree of the patient's sufferings and struggles. In the Dublin Lying-In Hospital, when under Dr. Collin's able care, out of all the women, 7050 in number, who were delivered within a period of two hours from the commencement of labour, 22 died, *or* 1 *in every* 320; in 452 of his cases the labour was prolonged above twenty hours, and of these 452, 42 died, *or* 1 *in every* 11—a difference enormous in its amount, and one surely calculated to force us all to think seriously and dispassionately of the effects of severe suffering upon the maternal constitution" (p. 113).

Of the dangers of chloroform Sir James Simpson thought very lightly. After once beginning its use at an obstetric case, he generally left its exhibition to be continued by the nurse, or by any intelligent friend of the patient who might be in the room. "Some of our midwives," he says, "use it in the cases which they themselves attend" (p. 146). Nor did he think the deaths, attributed to its use, were due so much to the anæsthetic as to the violence or shock of the operation; a position which he fortifies by several interesting examples (p. 151, 152) of deaths under operation where, had the anæsthetic been applied, it would undoubtedly have gained the credit or rather the discredit of the catastrophe.

Part III is occupied with lecture notes and personal experiments on various anæsthetic agents then new and unexplored, but which are too well known at the present day to require special notice at our hands.

The same may be said of much in Part IV, which treats of "the applications of anæsthesia in surgery and medicine." Directions for the exhibition of chloroform now familiarised by repetition are here found in their original form, and it is no mean proof of Sir James Simpson's accuracy and foresight that they should have undergone changes so slight and insignificant in a practice of twenty years. His summary of rules for the exhibition of chloroform in parturition may be adduced in illustration of what we have advanced:—

"Begin the inhalation of chloroform when the patient complains of much pain. This is generally towards the end of the first stage.

"2. Always inculcate perfect quietness around the patient, particularly when commencing to give the chloroform.

"3. Only give it during the pains, and withdraw it during the intervals.

"*Exceptions.*—Give a whiff of the chloroform also during the intervals when the pains are very severe, and the patient awakes complaining of them. Give small doses, or only repeat them every second or third pain, when the chloroform affects the action of the heart and uterus. These cases are very rare.

"4. When given during the first stage the anæsthesia need not be deep, unless the suffering be great, or the symptoms of anæsthesia disagreeable.

"5. As the second stage progresses, make the anæsthesia so complete as to destroy all sensibility.

"6. Do not allow the urinary bladder to become over-distended.

"7. Do not restrain the patient in one position.

"8. Be sure to remove the chloroform as soon as the child is born.

"Do not awake the patient artificially" (p. 206).

He seems to have given the preference to the simple handkerchief over all instruments then invented for the exhibition of chloroform; nor did he believe that any circumstance need interfere with the full effects of the anæsthetic, with the single exception, and that a doubtful one, of mitral disease of the heart. "This is the only affection where I have the least hesitation in administering chloroform. There is perhaps no necessity for this dread after all" (p. 182).

Many pages are occupied by cases illustrating the results of anæsthetic treatment—one of those at p. 187 being of considerable interest, as showing the safety which may attend the prolonged exhibition of chloroform, even in the youngest infants. A child being seized with convulsions within ten days of its birth, was about a fortnight subsequently "placed under the inhalation of chloroform, and kept more or less perfectly under its action for upwards of twenty-four continuous hours, with the exception of being allowed to awaken eight or ten times during that period for the purpose of suction and nourishment."

Many more pages are filled with the favourable opinions and comments of practitioners in various parts of the country, which perhaps might have been omitted without detracting materially from the interest or value of the volume.

Part VI treats of *Local Anæsthesia*, giving the results of experiments on the lower animals and on his own person. The devotion and fearless self-sacrifice of the operator are conspicuous on more than one occasion, and well exemplify the spirit in which all such inquiry should be made; but as the results were negative for the most part, the interest they possess is historical

rather than practical, and we may pass at once to the consideration of the second division of the volume.

II. *Hospitalism.*

If we consider the enormous interests vested in our large palatial hospitals, metropolitan and provincial, their prestige, their dignity, their traditions, and the pride with which they are contemplated both by the public and the profession, it will appear almost impossible that they should be abandoned or materially altered in external feature; yet if there be no fallacy in the figures collected by Sir James Simpson, no error in the conclusions he derives from them, we must look forward to changes in the future more sweeping and startling than any we have yet seen in our hospital system.

The papers on this subject will be remembered as occurring, some in the *Edinburgh Monthly Journal*, others in the *Lancet*, where the views they contain were warmly contested by Mr. Timothy Holmes and others. The champions on either side were worthy of the cause, but no impartial reader of all that was said and written can believe that the question has been set at rest, or indeed that it has been more than opened. In a review of this kind it would be impossible even to glance at the whole subject; we shall merely direct attention to the salient points in Sir James Simpson's argument, and hint here and there at a weak, or possibly erroneous, conclusion.

As early as 1848 Sir James Simpson wrote as follows in the pages of the *Edinburgh Monthly Journal* (November, 1848, p. 328):—

"There are few or no circumstances which would contribute more to save surgical and obstetric patients from phlebitic and other analogous disorders than a total change in the present system of hospital practice. I have often stated and taught that if our present medical, surgical, and obstetric hospitals were changed from being crowded palaces—with a layer of sickness in each flat—into villages or cottages with one, or at most two patients in each room, a great saving of human life would be effected; and if the village were constructed of iron (as is now sometimes done for other purposes) instead of brick or stone, it could be taken down and rebuilt every few years—a matter apparently of much moment in hospital hygiene" (p. 290).

And again, as president of the public health section at Belfast, and speaking before the National Association for the Promotion of Social Science, he argues that our hospitals should be changed "from wards into rooms, from stately mansions into simple cottages, from stone and marble palaces into wooden or brick or iron villages."

The statistics by which these conclusions are upheld are

derived from a careful analysis of the results which follow the four amputations through the bones of the thigh, leg, arm, and forearm, in large metropolitan or country hospitals, and in the rural homes of the poor respectively. Careful details were collected of 2098 such amputations in country practice, and of 2089 in the larger hospitals of the country, with the following summary of results:—

	Country practice.	Eleven large and metropolitan hospitals.
Total deaths	10·8 per cent.	41·0 per cent.
Thigh	18·3 „	46·5 „
Leg	13·2 „	44·0 „
Arm	4·3 „	37·0 „
Forearm	0·5 „	16·4 „

And a more detailed examination of the individual cases seemed rather to increase than diminish the appalling disparity which these figures exhibit.

1. That the country cases were not less severe, at least those treated after injury, seems proved by the number of successful double amputations after complex injuries, 7 having survived out of 23 operated upon (or 46 operations), whereas "out of a list of the last 11 double primary amputations performed in Edinburgh Infirmary, 10 of the patients died."

2. The success of amputations in the country on patients over seventy, as compared with the same in hospital practice, would appear to indicate causes which, as we shall presently see, Sir James Simpson persistently ignores. Immunity from hospitalism alone can scarcely account for the fact—if it is a fact, that "limb amputations are not more fatal to the old in rural practice than are the same operations performed upon people of *all* ages in our large and metropolitan hospitals" (p. 354).

3. Again, the surroundings of a crowded and often dirty cottage, with the obnoxious box-bed so common in Scotland, cannot be favourable conditions under which to earn the success which country surgeons claim.

4. Is there no difference in the physique and powers of resisting death? Sir James Simpson appears to think not:—

"The amputations in this class of injuries (traumatic) being necessitated by accident, the patients immediately beforehand must be held to be all similar in their state of strength and vital force. They were all alike in the condition of ordinary or normal health a few hours, or a very short time at least, before the operation was resorted to" (p. 373).

Much must depend on the answer to this question, and we cannot at present agree with Mr. Holmes in accepting *deaths from shock*, after amputation, as the best test of stamina or of debility of constitution.

The large proportion of deaths in country practice due to this cause, 36·4, per cent., and the small number in the Parisian hospitals, as compared with London hospitals, may possibly point to a different interpretation.

5. Sir James Simpson's position against large hospitals is further fortified by statistics which seem to show that the death-rate after amputation varies directly with the size of the building :—

e. g. In the large Parisian Hospitals	.	62 in 100 die.
In British Hospitals (300—600 beds)	.	41 „
„ „ (300—201 „)	.	30 „
„ „ (200—101 „)	.	23 „
„ „ (100— 26 „)	.	18 „
„ „ (25 beds or less)	.	14 „
In isolated rooms in country	.	11 „

But many circumstances would lead us to doubt whether the numbers from which the statistics are derived were adequate to the deduction of a true result. When the mortality, for instance, after amputation of the arm varies from 111 per cent. in St. Bartholomew's Hospital (p. 333) to 53·6 per cent. in Edinburgh, whilst it is 12·5 per cent. in Norwegian country practice; when amputation of the leg is followed by a mortality of 27·2 only at St. George's Hospital, while that of the arm (a much less dangerous operation) is 53·3 (p. 336); when at 'Guy's' amputation of the forearm costs 34·7 lives in 100 submitted to it, that of the leg only 35·3 (p. 335)—much caution will evidently be required in using figures so variable in the solution of important statistical problems.

6. And a similar remark would apply to the figures on which Sir James Simpson grounds his belief that

"The experienced country surgeon, operating upon his patients generally in cottages and villages is, as compared with the experienced city surgeon operating upon his patients in rich and magnificent hospitals, *five times more successful*" (p. 347).

That the mortality after limb amputation is greater in metropolitan hospitals than in country practice cannot be denied, but as an isolated fact it is useless unless we know the actual causes of death. Many and complex causes, social, physiological, and pathological, are at work to bring about the result as we see it. Pyæmia is not unknown in country practice, and isolated cases have occurred in hospital wards without any spreading of the disease. Still there can be no doubt that

"When two sick men are laid down in the same room, there is always a chance—slight, in many cases, it may be—of one of them deleteriously affecting in this way, by his exhalations, the bodily state of the other. When a sick or wounded patient is placed in a

room or chamber by himself, all such mischances from others are averted; and hence the advantages of perfect isolation of the sick. The danger however, on the other hand, no doubt multiplies as the number of patients aggregated together is increased" (p. 386).

And the question still demands an answer, whether by ample space, perfect ventilation, and greater care, the risks here alluded to may not be reduced to a minimum. Many points must be reconsidered and settled before we can finally condemn the noble buildings in which we all feel a pride, and accepting his conclusions, follow the advice of Sir James Simpson:—

"Build up the doors and any other entrances from the wards into the stair-landings, corridors, &c., and make all the wards and all the flats be entered from without, either by new external staircases, if the hospital were not above two stories in height, or by covered balconies or galleries placed upon the outer wall of the ward, and to which balconies or galleries entrance was given by the existing staircases. If every ward were thus prevented from sending its deteriorated air into the interior of the house, with the mischance of polluting the general atmosphere of the hospital, and if itself it had only communication with the external atmosphere, then each ward would become a separate cottage hospital, as it were, with a self-ventilation entirely its own."

III. Passing over the essay on hermaphroditism, we must say a few words, in conclusion, on (IV) the paper, reprinted from the 'Medical Times and Gazette,' on "Stamping-out Smallpox and other Contagious Diseases." As the poleaxe was the chief measure employed in stamping out rinderpest, so *isolation* is the chief measure proposed by Sir James Simpson for the stamping out of smallpox. Such a restriction of the liberty of the subject as is implied would be no new feature in legislation. Homicidal lunatics are prevented from destroying the lives of their fellow men, yet smallpox patients yearly destroy hundreds to their one. No harsh measures would be required; none, in fact, which are not now enforced in every well-regulated household when infectious disease assails any of its members. The regulations drawn up by Sir James Simpson are at once simple, straightforward, and practical:—

"1. The earliest possible notification of the disease after it has once broken out upon any individual or individuals.

"2. The seclusion, at home or in hospital, of those affected, during the whole progress of the disease, as well as during the convalescence from it, or until all power of infecting others is past.

"3. The surrounding of the sick with nurses and attendants who are themselves non-conductors or incapable of being affected, inasmuch as they are known to be protected against the disease by having already passed through cow-pox or smallpox.

"4. The due purification, during and after the disease, by water,

chlorine, carbolic acid, sulphurous acid, &c., of the rooms, bed-clothes, &c., used by the sick and their attendants, and the disinfection of their own persons" (p. 545).

These measures, if carried out faithfully and universally would, he thought, be sufficient to stamp out smallpox in Great Britain within a period of six months or a year.

Whatever we may think of the views held by our author, there can be no doubt opinions have changed even since the date of this paper (1868); and that what might then have been thought the dream of an enthusiast, is now reckoned amongst the possibilities, if not the probabilities, of the future. The progress of sanitary knowledge has been slow, but there has been progress, and the people of England are beginning at last to take some interest in the things which concern them most closely. Much, however, remains to be done not only in legislation, but in awakening a "sanitary sense" among the masses, before any such views as those we have been considering can obtain a fair hearing, or be carried to any practical issue. The problem present to the mind of Sir James Simpson is still unsolved; it is one of vast importance to the human race, and it cannot be better stated than in his own forcible words:—

"In the ten years from 1856 onwards there died in the United Kingdom, from scarlatina above 280,000; from measles above 130,000; from whooping-cough above 150,000; from smallpox upwards of 60,000; or about 600,000 of our population were killed off by these four diseases. To what extent can this terrible decennial death-roll be shortened or abolished by the process of isolation and stamping out? Is not the whole subject a grave and momentous question both for legislators and physicians?"

II.—Military and Naval Medical Reports.[1]

In our last notice of the annual 'Army and Navy Medical Reports' we studied them mainly with a view to considering what light they threw on the phenomena of yellow fever. This year we shall pursue a somewhat different course, and endeavour chiefly to glean and present to our readers the most important facts they contain, which bear on the subject of the geography of disease. We shall also have the opportunity of gathering

[1] 1. *Statistical Report of the Health of the Navy for the Year* 1869. 1871.
2. *Army Medical Department Report for the Year* 1869. London, 1871.
3. *Report on Barracks and Hospitals, with Description of Military Posts.* Washington, Dec. 5, 1870.
4. *A Report on Age and Length of Service, as affecting the Sickness, Mortality, and Invaliding in the European Army.* By James L. Bryden, Surgeon, Bengal Army, Statistical Officer, &c. Calcutta, 1871.

some hints on the use of quinine and other antiperiodics, especially as prophylactics. We shall conclude with a very few remarks on some of the conditions which influence the health of European troops in India.

The chief novelty in the naval reports consists in the information which they contain concerning the sanitary conditions of the great home establishments of the service, the naval hospitals, marine (divisions), and the dockyards, and which is now supplied for the first time. For the home stations the reports on those establishments are indeed more interesting than the ordinary ones, which for this year do not record much of importance besides accounts of some attacks of measles, smallpox, and scarlatina, fortunately attended by but a small mortality. In the Report on Sheerness there is an instructive account of a disease formerly common in London, now exceedingly rare; 189 fresh cases of ague occurred at Sheerness during the year. Dr. Forbes writes that although malarious poisoning thus prevails to a great extent—even infants at the breast showing its effects, it is seldom that a case of fully-developed ague running through its three stages occurs. By far the greater number of cases are of irregular ague, undeveloped or dead ague, as it is loosely called. There is all the languor and depression of the cold stage, but no well-defined hot or sweating stage. A point of much interest is, that the only visceral complication is found in the liver. Affections of the spleen, so common in the fenny and other aguish districts, are unknown in Sheerness and its immediate vicinity, though at the south-eastern end of the Isle of Sheppey they occur in the old farm-labourers, who had contracted ague in boyhood. In adults the ague had a tendency to assume the quartan type, among children the quotidian. As regards treatment a brisk emetic often cuts short the first stage. Quinine of course was the main remedy, but change of air was often required.

The Return for Devonport affords a good example of the occurrence of autumnal diarrhœa, 87 cases having occurred during the quarter ending September 30th, which part of the year had been characterised by very hot weather. Many cases were attributed to unavoidable exposure to the sun, but many also to the unwholesome character of the food eaten—especially cheap fish or shell fish when not over fresh, the injurious action of which is supplemented by the use of cheap and unripe fruit. The attacks came on either early in the morning or after the midday meal. The treatment was simple, and there were no casualties.

Three cases of cholera, with one death, occurred in the home station force. There is nothing remarkable in the cases, except

their close resemblance to the Asiatic form of the disease, and their being entirely isolated.

In the Mediterranean station an outbreak of what was called influenza enterica, of which there were no fewer than 198 cases on board the *Caledonia*, was the most noteworthy occurrence. The crew brought out in the *Revenge* to join the *Caledonia* (which it did at Malta in the end of April) suffered on its way out from a form of febrile catarrh, accompanied by sore throat of a low type, and by great prostration. The men were transferred to the *Caledonia* with a low typhoid influenza hanging about them, but nothing remarkable happened till after the ship had left Malta for exercise. On the 16th of June, on their return from eleven days' practice, when near Malta, at 11 p.m., nearly fifty of the ship's company and some of the officers were suddenly attacked with diarrhœa. Besides more general symptoms of debility, in the great majority of instances the patient had a red, irritable tongue, and erysipelatous redness of the whole buccal mucous membrane. Such individuals had invariably a troublesome diarrhœa; in a few attended with nausea and sickness; and in nearly all with irregularly-occurring pyrexia. It was observed that attempts to check the diarrhœa were always followed by exacerbation of the fever. Mild laxatives, with nourishing diet, was the treatment found most efficacious. Fortunately there were no casualties. Curiously enough the termination of the epidemic was characterised by the return of the symptom which marked its commencement on leaving England, a doubtful form of cynanche, and tendency to lung complication. Staff-Surgeon Cotton in his very intelligent report looks on the whole epidemic as a manifestation of typhoid poisoning. He believed that the disease acquired infecting properties from the concentration of sick individuals as well as from a high temperature, and he thought the period of incubation short. The disease here described differed very widely from ordinary enteric or typhoid fever, and there is still much to be explained respecting the causation of that fever, especially as it occurs on board ship under a great variety of conditions.

There were a few cases of remittent fever and of ague in this command, but they do not call for any special remark. Here is Assistant-Surgeon Bolton's opinion of quinine as a prophylactic. "As such it is most necessary, especially when men are employed in or near swamps, or upon boat service; but it must be given in large doses, six grains night and morning, combined with some stimulant, such as sherry or rum. I know many old stagers on the Danube works who would sooner go to their duty without their dinner and tobacco than without their quinine. If they should even for a day forget their allowance, they are

almost certain to feel indisposed, or have an attack of ague in a few hours."

There were twenty-five cases of worms, chiefly of tænia, and attributed to the consumption of unwholesome pork got at Malta. The male fern and kousso were usually found efficacious.

The Reports on the North American and West Indian, and on the South-East Coast of America, introduce us to yellow fever. There were 106 cases, with 52 deaths. The accounts of the yellow fever are excellent, though there may not perhaps be much in the account of symptoms, or in treatment that is new. Assistant-Surgeon Thomas, who fell a victim to the disease, showed admirable self-denying thoughtfulness for his charge, in privately communicating to his commanding officer his suspicion as to the nature of the disease with which he had been attacked, and inducing him to conceal its nature, so as to prevent unnecessary alarm. One observation is singular:—"It did certainly appear that the yellow fever poison had less hold in a debilitated and worn-out constitution; and the robust and healthy constitution proved to be the aptest instrument for the development of this virus, and the best pabulum for it to feed on." We believe that in the case of most epidemic diseases we are far too apt to assume that they have a preference for the weakly. We know but little of what constitutes predisposition.

Some observations on the nature and origin of yellow fever by the Deputy-Inspector-General of the Naval Hospital at Port Royal, Dr. Donnet, of which we give the substance, are well worthy of consideration, although entering on the path of speculation.

The history of the fever which occurred in 1869 would seem to favour the theory that the germs which generate the epidemic had their origin in the marshy and swampy grounds in the vicinity of the locality where the ships that became infected were anchored, and the choice made of these ships, to the exclusion of the others in the harbour, assisted towards confirming this belief. If therefore this theory be accepted, we must bring ourselves to admit that yellow fever may have its birth in localities such as this: that its origin may be due to some effluvium, germ, or organism generated on the surface or within the bowels of the earth; and, if so, that it finds a habitat in such places, and that therefore yellow fever is endemic in the island of Jamaica. But this noxious principle differs *in toto* from that which produces simple continued fevers and the types of remittent fever, for each of the various West India fevers has a distinct type, characteristic of an individual poison; for in all epidemics of yellow fever an individuality has been observed which has been constant. The epidemic of yellow fever which happened in

Lisbon in the year 1723 differed in noways from that which devastated the same city in 1857. There do, however, sometimes occur in Jamaica—modifications of fever which obscure their character, and render them less defined. These modifications may be accounted for by the admixture and absorption into the system of other poisons besides that of yellow fever, and may run a parallel course along with them.

But let us turn from these doubtful and complicated generalisations, to the practical fact laid down in the report, and unfortunately owing to various considerations of what are considered the exigencies of the service, not sufficiently often carried out, so that it cannot be too often repeated, or too strongly stated, that the only chance for a ship's company amongst whom yellow fever has made its appearance, is to have them conveyed with the utmost speed to a cold latitude.

The mortality from yellow fever was about 50 per cent., or nearly twice as great as it usually is. This is supposed to be accounted for by many of the milder cases having been returned as continued fever—a fact that militates against the opinion so strongly expressed above, of the symptoms of yellow fever being always marked so unequivocally.

There is not much to remark on the South-east Coast of America station, except that the medical men at Buenos Ayres, and two of the medical officers of the Paraguayan army, did not think there was much use in employing quinine as a prophylactic; that the men suffered extremely at Paraguay from the attacks of mosquitoes and other insects, and that a single fatal case of malignant cholera occurred in a boy. The case had all the usual symptoms of Asiatic cholera, but could be traced to no source.

There was a fatal case of tetanus in a young seaman, supervening, as most attacks usually do in the tropics, on a slight wound, in this instance of the great toe. The first symptom in this case was epigastric or diaphragmatic pain shooting to the spine. According to the experience of Buenos Ayres such a pain indicates the onset of tetanus, whereas præcordial distress indicates pyæmia. Tetanus is common in Paraguay in the cold season.

The diseases of the Pacific station suggest the inquiry, Why, in what is generally a healthy station, rheumatism should be common, for in this station and also in the China one, we find rheumatism more prevalent than in Europe? In the Home station it was 48; in the Pacific and Australian, 85; and in the China as high as 92 per 1000.

In the preceding year the ratio in the Pacific—also in Australia—had risen to 105. No cause is assigned for rheumatism being so frequent; but it is said to have been generally

met with in the subacute form, which is by no means amenable to treatment. It runs a tedious course, and generally produces, or is accompanied with, a low state of health; heart complications appeared only in one case.

We once had occasion, in India, to observe several cases of the most acute rheumatism in a regiment which had just arrived after a voyage from Australia. Many suffered from it during the voyage, and the regiment may almost be said to have brought its rheumatic tendency with it. In many instances there was heart complication, and fatal pericarditis in one.

The cases which occurred in the China station during this season were more acute than those which were treated in the Pacific; but here also there were no heart complications. In the same season in the Red Sea there were instances of heart affection, and this was set down to the high temperature, while the attacks in Bombay Harbour were attributed to damp air. In the Cape and West African stations again the ratio rose to 120·8 per 1000. This is explained by saying that the majority of these cases were the sequelæ of remittent fevers. This is scarcely a satisfactory explanation, except for a portion of the cases; and in fact we know as yet of no good reason why, in such favorable climates as those of Australia and the Cape, rheumatism should be so much more common than in Europe.

Amidst the luxuriant vegetation of the West Coast of Africa so rife with fever, we meet with a rather unsatisfactory notice of the use of quinine as a prophylactic. "Whilst we lay at Punta da Linha a merchant ship had been lying moored to the bank of the river; she had been three weeks there, during which time none of the men had taken quinine. I advised the captain to issue the quinine at once, which he did. However, soon after this all his men were attacked with fever. A party of men from the *Fly* were employed on shore. These men received five grains of quinine before going on shore, and also on their return in the evening. Nevertheless, most of them contracted fever soon afterwards." For the remittent fever quinine was the main remedy; its first dose being often given with Epsom salts, a very excellent combination. Gastric irritability had to be subdued.

The further treatment of remittents on this coast seems to have been chiefly by the exhibition of moderate doses of quinine, and of alcoholic stimulants, when there was much depression.

"In the last outbreaks," writes Surgeon Magill, "I have never given a larger dose than five grains, and have not repeated it oftener than three times in the twenty-four hours; and as soon as the acute fever has subsided, have not given more than one five-grain dose, and the cases have been more tractable. Vomiting took place in

some cases before they came under treatment, but it has not occurred in any case during treatment, except after a large dose of quinine, or quinine too long persisted in. I have no hesitation in acknowledging my belief that some cases owe their protraction, in some degree at least, to the injudicious persistence in the use of quinine."

The chief interest in the report on the East India station centres in the account it gives of cholera at Zanzibar. Every possible condition of bad sanitation appears to have been present in that place to favour the spread of the disease, when once introduced, and that it was brought over in dhows from the mainland of Africa, seems certain. The question of interest is, how it reached Pangan, the point on the main land opposite Zanzibar. In the month of September, 1869, the presence of a very fatal disease in the Masai country was ascertained; from thence it reached Pangan, and got to Zanzibar by the end of November, 1869. The fact of the disease travelling down to the coast from the Masai country is clear. But how did the disease first reach that country? It has been supposed that it may have got to it from the Red Sea, and it has been conjectured even that cholera may have arisen *de novo* in Central Africa, but nothing is satisfactorily known on the subject. The disease is believed to have reached the west coast of Africa by means of a caravan from Morocco. It may in like manner have reached the interior of the country from the Red Sea. On the whole there is much to throw doubt on one of the dictum of the Constantinople Cholera Conference, that cholera was always extinguished, when caravans having it crossed through deserts.

The immunity of the Seychelles and other islands in the neighbourhood from cholera, was attributed to the maintenance of a strict quarantine.

There were fortunately only three cases of cholera, two of them fatal, in the ships on the East India station. A boy embarked at Zanzibar on November 29th, was seized with cholera at sea on December 2nd, and eventually recovered two days after, or on the 5th. The man who had attended him was taken ill and died. This man had never been ashore at Zanzibar. The disease did not spread further, whether owing to the precautions adopted, Dr. O'Connor very judiciously entertains a doubt. In the other fatal case the officer went ashore at Bombay on the 9th, returned and retired to bed at ten p.m.; was seized in the early morning with vomiting and purging, and after a struggle of two days died on the morning of the 12th. These are fair average specimens of cholera on board ship, when it is not epidemic.

Closely allied to cholera was a very curious case of diarrhœa and vomiting, which was complicated with a loss of power in the extremities. The patient was seized on the night of the 13th, and the case was protracted over six days. It is well known, that in many parts of India horses are often suddenly seized with paralysis, which is usually attributed to the land winds. In the human subject both paralysis and tetanus have been ascribed to similar causes. But in the present instance the history of the onset and of the first day of the attack seems to point distinctly to cholera.

The China station report affords some glimpses at the seasonal prevalence, as well as at the geography of disease. For instance, we find that smallpox was so very prevalent at Shanghai, in winter, that Europeans had to be cautioned from going into the native town. Again, we find that at Yokohama it is usual for typhus fever to prevail in the early months of the year, both in the foreign settlements and amongst the European population.

With respect to Queensland, its coast has generally been considered free from malaria, and was believed to be very healthy; but experience proves that here, as well as elsewhere within the tropics or near them, where large tracts of mangrove swamp exist, unhealthy seasons do occur, and produce remittent fever. Nevertheless, writes Surgeon Crosbie, there can be no denial of the small quantity of malaria produced in proportion to the conditions capable of generating it. The Northern Queensland towns are placed in localities which in the West Indies would be utterly uninhabitable by white men. No attention seems to have been paid to sanitary conditions, and, in fact, the first settlers would almost seem to have been attracted by extensive swamps, yet the inhabitants do not suffer from remittent fever to any extent.

Problems of this nature are well worthy of further investigation, as well as the immunity which New South Wales, in common with the Cape of Good Hope, has, up to the present moment, enjoyed from the ravages of pestilential cholera, especially when we consider the rapidity and the constancy of intercourse between New South Wales and the whole of the East. The slight outbreak at Swan River, in 1831-32, can scarcely be regarded as an exception to the general fact of the immunity of those countries up to the present period. We must pass by the wider question, whether any locality can be supposed to enjoy a permanent immunity from cholera, as has been imagined by some.

We have now run through all our naval commands; but on one subject we have not touched, on which these reports afford

much information. One general impression that they leave is, that of the advantage everywhere accruing, even from imperfect application, of all measures directed against the spread of the contagion of syphilitic disease. Wherever in any port prostitution is kept at all under the eye of the authorities, our sailors suffer less from syphilis.

We can just allude to some subjects touched on in the appendix. Staff-surgeon Jenkins, in his report on the health of the artillery division of the Royal Marines, offers some very sensible observations on their dress, especially with reference to diseases of the circulatory system. He thinks it can be easily demonstrated that many of these affections have a mechanical origin.

Dr. Macleod's report on the Royal Naval Lunatic Asylum at Great Yarmouth contains much excellent matter; but we have only space to remark, that he makes out the proportion of insanity among sailors to be much the same as among other classes of the community, perhaps a trifle smaller; and to add that we entirely agree with him in his comments on the smaller hopefulness of the results of treatment in cases in which there is not some pretty active delusion, and on the entire neglect with which insane patients are usually treated by their relatives, unless they happen to possess some income of their own. His observations on the soothing effects of aperients in irritable cases of insanity are also perfectly just.

The essay on heat as a physiological, pathological, and therapeutical agent, by Dr. Rattray, contains many valuable generalisations on an important subject, but we cannot enter on them; the same officer has laid some excellent observations before the Royal Society on the pulse, respiration, and temperature of sailors at home and in the tropics.

What we may call the companion volume of the Army Medical Department for the same period, contains fewer notices of points of interest connected with the geography of disease; and it does not, indeed, contain any local reports of maladies containing much novelty. The strength of the *Medical Report of the Navy* lies in various elaborate essays in the appendix.

The account of cholera on the Western Coast of Africa calls for no special observations; but the impression that it reached the French settlement in Senegal by means of a Moorish trading caravan is worthy of attention. The point of importance is, that cholera seems at this time to have reached both the Western and the Eastern Coasts of Africa from the interior. Cholera was fatal in fourteen out of twenty-one cases in the black troops at Gambia, thus showing the disease to be at least as fatal in Africans as in Europeans.

In the sanitary report on Sierre Leone we read, that the

natives suffer little in comparison with Europeans from malarious disease. Chest affections, rheumatism, scrofula, and leprosy, are the disorders most frequently observed among them. The only one peculiar to them is *lethargus*, to which they occasionally succumb, and of which it would be desirable to have further accounts. The epidemic diseases are yellow fever, variola, and catarrh. Of these the first affects almost solely the Europeans, and appears usually at the end or at the commencement of the rains. Variola is endemic, and occasionally becomes epidemic. Acute dysentery or diarrhœa are rare.

In the Mauritius the epidemic malarious fever, which began to prevail at the end of 1866, continued to prevail with more or less fluctuation up to the end of 1869.

In Japan we have confirmation of the statement in the naval reports concerning smallpox, viz., that the disease is very prevalent every cold season, and again at the end of winter. The fevers most commonly met with are typhus and enteric; the latter (and very possibly the former) is probably in a great measure due, or at least is attributed, to the custom of collecting in open tubs ordure and urine for manure, which are kept near the doors and create an inconceivable stench.

The climate of Singapore is well described by Assistant-Surgeon Lindsay, as marked by constant moist high temperature, with no true hot weather, and no cold season, as in India, and producing fevers slight in the severity of individual cases, but serious from their frequency and their debilitating consequences. Diarrhœa and dysentery are frequent, but not of a bad type. Dyspepsia is common. The equability of the seasons and the constant high temperature cause a feeling of enervation and of languor, which can only be remedied by change of climate.

There is a notice of beriberi (that unsatisfactorily defined disease) at Labuan, and we have to regret that more is said of the circumstances which induce that condition of debility than of its actual nature, the more particularly as in these days some are inclined to doubt the existence of beriberi as a separate affection, and rather regard the symptoms which characterise it as sequelæ of fever of the intermittent type.

The climate of Labuan closely resembles that of the warm summer months in the south of Ireland. The temperature is far lower than in most of our Eastern possessions. It varies very little during the year, the extreme ranges being from 71° to 90°. The rainfall of 1869 was 182 inches, but this is much in excess of the usual rainfall. The chief rainfall is from May to October, and most of the rain falls at night.

There is little of importance in the returns for India for this year. There is a careful inquiry by Surgeon Thompson respect-

ing an outbreak of cholera at Thyet Myoo in Burmah. His replies to some of the chief questions in the printed instructions for conducting an inquiry into cholera in India are stamped with good sense and judgment.

It may be conveniently mentioned here that Dr. Fleming has continued his researches on the intimate nature of Delhi boils—a variety of ulcer known in various parts of the world by various local designations. He has proved that pus alone from the Delhi boil will not propagate the disease, but that certain brown-yellow cells, presumably therefore of a specific kind, do so. The only effectual mode of treatment is to destroy the cell-structure in various ways, as by a strong caustic solution, potassa fusa, or nitrate of silver, and when the growth is destroyed, to treat the resulting ulcer on ordinary principles. As it is not yet known how the specific cells arise which cause the boil, the preventive treatment of the Delhi boils is still undetermined.

There is a careful report on the epidemic fever at Trinidad by Staff-Surgeon Thomas. He found that the general tendency of ague when present appeared to be to pass into remittent, and of remittent into yellow fever, and each of these diseases presented deficiencies and anomalies in their symptoms. Notwithstanding that the diagnosis between specific contagious yellow fever and the malarious form of yellow fever is at all times difficult, Dr. Johnston has no doubt as to the separate existence of the former. And the general result of his experience in the late epidemic convinced him that he had lately had under notice in that locality the following varieties of fever, partially concurrent or intercurrent:—1. Simple continued fever; 2. Ague; 3. Remittent fever; 4. Malarial yellow fever; 5. Specific yellow fever.

It was observed as a peculiarity of the epidemic, that not only as a rule did quinine appear not to exert its usual beneficial effects, but its exhibition tended to increase the sufferings of the patients. Somewhat incredulous, very naturally on the point, Dr. Johnstone administered quinine when he had opportunity, but in no instance with satisfactory results, until near the subsidence of the epidemic.

We have thus traced some of the salient points of these reports, chiefly of those bearing on the diffusion of tropical disease, and quit the subject with the expression of a hope, that we shall some day soon have a full account of the clinical experiences of Netley in the treatment of men who have returned labouring under the effects of disease contracted on foreign service. The want of this is not made up for by the observations on the matters suspended in the land and sea air at Netley—of which the results may be thus condensed:—

In the first operation on air from the land the type of the

impurities was the preponderance of vegetable cells and fibrous pollen and fungi; in the second, or in the experiment with sea air the particles were the same, but the vegetable products were much diminished. The typical floating bodies of barrack air seem to be large globular cells, ragged nondescript filaments and fibres from the clothing; whilst in the air from the phthisis ward, there were found,—what had not been observed in the other examinations,—forms like pus corpuscles, and to all appearance the peculiar cells of tubercle; and it is not difficult to suppose that the existence of these structures in the atmosphere, and their inhalation may tend, under certain conditions, to the propagation of that disease by what may be called infection, which has from early times been believed in by some.

Almost the only scrap of information connected with practical medicine we get from Netley is from Dr. Beatson, who informs us that many of the men invalided on account of tropical dysentery have had their symptoms greatly aggravated by change of temperature in spring on this side of Alexandria, and that patients labouring under pulmonic affections have suffered from this cause, the deaths from pneumonia being frequent on board ship, at Netley, and in hospital at Portsmouth.

The third volume on our list, the *Report on Barracks and Hospitals throughout the United States*, is a valuable record for use in America. It is generally of a topographical nature, and only alludes incidentally to the diseases of the various districts. Surgeon Cooper's report on Fort Monroe is the fullest in this respect. There are some curious notices of the annoyance caused by reptiles and insects in some places. We shall endeavour to cull a few facts bearing on the geography of disease, and on the use of certain remedies in a way not familiar to us.

At Fort Monroe, in Virginia, we hear of a disease that is particularly prevalent in America (and why it has been so is unexplained)—cholera infantum. It is described as common at that place, and often of a severe type. On account of it the second summer of a child's life is looked on with much anxiety by mothers. Until the summer of 1869 there was no disease that gave the medical officers so much trouble and anxiety, and that was treated with such unsatisfactory results. There was no disease offering a greater mortality. But since the disease has been treated with bromide of potassium, according to the suggestion of Dr. S. Caro, of New York, it has ceased to give trouble! Bromide of potassium is considered as certain a cure in cholera infantum, as quinine is in malarious disease, and is prompt in its action. This is undoubtedly an employment of the bromide, that one would scarcely have thought of.

Since the civil war a remarkable change has come over Fort

Monroe. Previous to that time it was emphatically the great watering-place of the Southern States, and many who suffered from malarious cachexia came there to "recuperate their health." Now, however, malarious disease is quite common. Two hypotheses have been advanced to explain this. One that the lands under cultivation have ceased to be well drained and well cared for. Most of the country was formerly well covered with virgin forests, which were supposed to intercept winds coming laden with malarious exhalations. The greater part of these forests has been cut down, and most of the land brought under cultivation, a process which is believed at first to engender fever.

The other hypothesis, which finds many supporters is, that as large quantities of clay and soil have been brought into and around the fort, to erect and to repair military works, the spreading out of this clay and soil engenders the fever.

The value of serpentaria, and the superiority of it to quinine in convalescence from malarious fevers, is strongly insisted on, especially when combined with some of the preparations of iron. The serpentaria appears to ward off the relapses so common after remittent fever. Serpentaria is also recommended as a prophylactic against the effects of malaria.

It may be worth while to mention that elsewhere the Cedron bean is said to be a most efficacious remedy in the treatment of intermittents. It requires only from ten to thirty grains to break up an attack. In its action it was entirely free from any of the disagreeable effects of quinine. The relapses were fewer than when the disease was treated with quinine or with chloride of sodium. It also is used as a prophylactic. Our own belief is that it is very uncertain whether any drug can be regarded as prophylactic against malaria.

Twenty-four cases were treated with chloride of sodium in drachm doses, given every hour during the period of intermission for four consecutive hours before the expected paroxysm. The disease yielded readily to this remedy, and but few of the cases required more than one or two days' treatment with this cheap but very reliable anti-periodic.

A peculiarity at Fort Monroe is the almost universal appearance, about the seventh day after delivery of a severe chill, which is the first symptom of a remittent fever that afterwards develops itself. With this fever come considerable tenderness of the abdomen, severe uterine pain, and some symptoms resembling those of puerperal fever; but they all yield promptly to free doses of quinine.

The effect of locality is well illustrated by comparing the returns for the various ports round New York harbour. Thus at Fort Hamilton malarial fevers are very prevalent. At Fort

Schuyler there is little fever, and more diarrhœa and dysentery; and this applies to West Point, where both the cadets and the soldiers suffer to a very considerable extent from those complaints. At Atlanta, Georgia, there seems to have been an epidemic of cerebro-meningitis; but we have no details of it. The place is said to be particularly unhealthy for children, who die in great numbers, chiefly from bowel complaints; at Forts Jackson and St. Philip, Louisiana, while coloured troops garrisoned the port, intermittent fevers were comparatively rare; but since the advent of white troops, there has been a marked increase of those diseases.

What is said of remittent fever has a considerable bearing on questions now agitated in India, as to the relations between remittent and typhoid fever. It is now maintained by many that the ordinary fever, which is so common in India in their first hot weather in men newly arrived, is typhoid fever. The fever has always occurred, though it has not been described as typhoid. If it be true typhoid, which we doubt very much, it seems to be quite independent of what are usually considered to be the sources of typhoid poisoning in England.

At Baton Rouge, Louisiana, in July and August, the fevers are more of a remittent character, and frequently congestive. In September the fevers are of a severe congestive or typhoid type. We shall quote, in a condensed shape, what is said by Assistant-Surgeon Patzki, of Fort Richardson, Texas:

"The duration of the remittent fevers varied from a few days to three weeks and longer. The resemblance of the protracted cases to typhoid fever was very striking. The symptoms distinguishing it from typhoid are, absence of eruption, the most careful examination failing to discover rose spots (sudamina were seen in one case). Clearness of the sensorium during the whole course of the fever, freedom from stupor, coma, or delirium, except in one case; absence of bronchitis, or of pain in the ilio cæcal region. The tongue was heavily coated, but remained moist throughout. The pulse not so frequent as in typhoid, exceptionally rising above 100. The temperature ranged from 100° and 102·5° in the morning to 101·5° and 104° in the evening. In a few cases the course of temperature was that of a mild case of typhoid, closely resembling Wunderlich's ambiguous kind of typhoid fever. Gastric and intestinal irritation were marked with some tympanitis. The stools closely resembled the ochrey stools of typhoid. The debility was great, the convalescence slow, all but two cases relapsing, one case twice, with fatal termination. The relapses occurred from two to eight days after apparent convalescence. The post-mortem in the only fatal case revealed meningitis, hepatization of lungs, engorgement of liver and spleen, with well-marked inflammation of the lower portion of the ilium, extending into the ascending colon. Peyer's glands were

slightly prominent, but there was no ulceration or cicatrization. Mesenteric glands were enlarged."

Scurvy is a disease not unknown at some outposts. The scorbutic taint was attributed to the hardship and privations from the severity of winter and a constant pork diet. Want of variety and of fresh vegetables no doubt contributed at many outposts to cause scurvy, which in some it is said manifested itself with terrible malignancy.

The distribution of disease, according to altitude and in different zones, is a subject of considerable interest, but of which very little is known. We have been able to glean a few particulars bearing on it from the American reports which, in the general dearth of information on such subjects, are well worthy of attention.

Camp Grant, Arizona Territory, lat. about 33·5 N. long, 111·10 W.; has an elevation of about 2500 feet above the sea. There is a large amount of malarious fever.

Fort Colville, Washington Territory, latitude 48·41 N., longitude 117·55 W.; altitude above the sea, 2800 feet. Phthisis is very prevalent and very fatal among the Indians. But two or three cases of the disease which were imported, improved rapidly under treatment.

Fort Laramie, Wyoming Territory, latitude 42·12 N., longitude 104·31 W.; altitude 4519 feet. In 1849 cholera was on the plains, but did not advance beyond; in May, 1859, cholera made its appearance, but there were not many cases.

Fort Bridger, Wyoming, latitude 41·18 N., longitude 110·32 W.; altitude 7100 feet. The post is generally healthy, but a periodical form of fever prevails among the mountaineers, known as "mountain fever." It has all the characters of remittent fever, and yields readily to the usual treatment; but if neglected the patient rapidly sinks into a typhoid state and dies.

Fort Sanders, Wyoming, latitude 41·13 N., longitude 105·30 W.; altitude 7161 feet. The post generally is healthy. The milder forms of affections of pulmonary organs are pretty frequent, owing to the high winds and dust; more deep-seated affections are rare. It is believed that owing to the wind the climate would be unfavorable to phthisis.

Fort Garland, Colorado, latitude 37·22 N., longitude 105·23 W.; stands at an altitude of 8,365 feet. Climate dry and most favorable to all diseases of the respiratory and digestive organs. Some curious statements are made respecting the influence of its climate on the uterine organs. Hæmorrhagic labours are common. Abortions, miscarriages, and menorrhagic affections are frequent. The females of San Luis Park are two to three years later in menstruating than those of the same race in the plains. It is

not uncommon for Mexican and Indian girls to be married before they have menstruated. In one instance a female did not menstruate till after the birth of the fourth child.

Fort Fetterman, Wyoming, latitude 42·8 N.; longitude 105·7 W.; altitude 8500 feet; is healthy; the chief diseases have been those of the throat and the eyes, attributed to dryness of air, frequent wind and dust, and occasional sporadic cases of malarious, rheumatic and pulmonary disease.

Having thus glanced at the hints afforded by these several reports on the geographical distribution of disease, we shall conclude with a few remarks on a subject of much importance to the large portion of our army, which serves in India.

The elaborate report on hygiene by Professor Parkes, the complete essay on hospitals, on the dress of soldiers, on ophthalmia, and on other subjects, are well worthy of study, but could only be adequately noticed in a review of each special subject. We shall merely enter slightly on one question, of much influence on the health of soldiers—their treatment on first arriving in India, and the regulations respecting invaliding; both subjects touched on by Drs. Beatson and Muir.

The latter of these officers comments severely on the number of boys sent out to India; but, as a matter of fact, it is satisfactory to know that little more than 4 per cent. are under the age of 20, as is shown conclusively by Dr. Bryden, in the report placed in the heading of this article, which deserves fuller consideration than we can give it here.

There seems to be no question that a large number of young men in a regiment—that is of the ages from twenty to twenty-five—usually produces for the first two or three years a very large sick list, but fortunately not a corresponding mortality. Young men suffer in excess from acute diseases, but they fall short in their attacks of chronic disease. They die in much the same proportion as older men of fevers, but much less of heat apoplexy and of hepatitis. On the whole, a too large proportion of young men may for the first two years favour inefficiency, but it does not produce the amount of mortality usually attributed to it, although it predisposes to disease at a later period. Nothing is more important for the prospect of an individual or of a regiment being fit for Indian service, than that they should escape sickness for the first two years, until they get into the ways of the country, until they are acclimatised (*i. e.* so far as any one is acclimatised). It seems to be ascertained that regiments new to India suffer much more severely from cholera than older ones. It is, therefore, of infinite importance that they should be specially cared for, should arrive in India at the proper season, if possible be not sent to a station where they are likely

to be exposed to cholera, and be sent as speedily as practicable to a hill station. All Indian authorities are agreed on this head, although they differ as to the number of regiments to be kept in the hills. Our own impression is that the main bulk of the army must always, for strategic reasons, remain in the plains, and the mortality need not be very large, if only cholera epidemics can be avoided.

Our own impression further is, that we have been in too great a hurry in building our new barracks, and in erecting them on uniform principles, without much reference to local conditions of climate. Unnecessarily large sums have been expended by Government, the expenditure increased by carelessness of construction, in such matters as mortar; and the men are by no means gratified with the result. Nevertheless, in some instances, as the lofty Dalhousie Barracks in Fort William, large barracks have answered every anticipation.

The less men are aggregated together, the better their chance of escaping epidemic diseases, and the greater the likelihood of men doing something for themselves and finding some occupation.

Dr. Beatson remarks very judiciously, that invaliding committees should not assemble at the periods fixed in former years, when journeying in India occupied so much longer time than now; and he thinks that in many cases, if the committees did not assemble till the 1st of January instead of the 1st of October, the preceding cold weather would have recruited the health of many men to such a degree, that it might not be necessary to send them to England at all that season.

When invalids are once sent down to embark, they should not be delayed in the port of their embarkation—a fruitful source of mortality. Invalids should certainly not arrive in England until the end of April or May, and all are agreed that the best time for arriving in India is the last two months of the year.

Regulations should be framed as much as possible to meet these propositions, which are generally granted. They can be carried out, save under exceptional circumstances, not, however, without considerable modification of existing arrangements.

III.—Sir H. Holland's Recollections of Past Life.[1]

A FEW days after we had received Sir Henry Holland's 'Recollections,' we accidentally met with a short notice of the well-known Chevalier Taylor in the 'Biographie Universelle

[1] *Recollections of Past Life.* By Sir HENRY HOLLAND, Bart., M.D., F.R.S., D.C.L., &c. &c., President of the Royal Institution of Great Britain, Physician in Ordinary to the Queen.

Classique,' which seems so singularly applicable to the volume before us that we will present it in full to our readers. "A famous English oculist of the 18th century, who died apparently in Paris some time after 1767:—He several times travelled over all the countries of Europe, and was honoured and generously rewarded by kings, princes, and the Pope; but he injured his own reputation by his incredible vanity. He published in his own name, 'Anecdotes of the Life of the Chevalier Taylor,' &c." The writer then mentions two of Taylor's principal works, one of which was translated into *eight* languages. We are not aware that either of Sir Henry Holland's former works has obtained any such amount of celebrity as this one of Taylor's, but we think his long and eminently successful career has been quite sufficiently remarkable to justify him in taking the unusual step of publishing a sort of autobiography. We could indeed have wished that the style of some parts of his book had been different; moreover, we are not quite inclined to endorse the opinion which has been expressed to Sir Henry himself by some friends, and which he "welcomes as the best justification" for the publication of his narrative, viz., "that there is much in it that may be practically useful to those entering upon life, or going through its later stages" (p. vi). For, to say the truth, the chief lesson inculcated in the book seems to us to be the importance of devoting two months regularly every year to foreign travel, a lesson which Sir Henry himself (owing to "the liberality of an excellent father, and [a very exceptional amount of] early professional success" (p. 3) has been able to put in practice for more than fifty years, and which he again and again dilates upon with pardonable satisfaction. However, it falls to the lot of few only of his less fortunate professional brethren to commence so early in life, or to continue holiday-making with such regularity. But whether the book is or is not as "practically useful" as Sir Henry was led to expect, there can be no doubt as to its being remarkably interesting and amusing both to the general public and to the members of his own profession.

Any physician who has for more than fifty years been attending persons of the highest rank, even if he has not the active and observant mind of Sir Henry Holland, must almost necessarily have collected a rich store of reminiscences. Sir Henry has made full use of his opportunities in this respect, and his book is so full of anecdotes and notices of many of the most eminent persons of the present century both in this country and on the continent, that it is difficult to make any selection amidst such an *embarras de richesses*. Of Sir Henry himself perhaps we do not learn much more than was known

already, except that many of the present generation of physicians are not aware that he was, when quite a young man, engaged (through the instrumentality of Mr. Keppel Craven and Sir William Gell) to attend upon the Princess Caroline of Wales as physician, during the first year of her residence on the continent in 1814. This was the means of enabling him to see many parts of the continent with very unusual advantages, and also of introducing him to new friendships, which were valuable to him in every way in after life (p. 114). From accidental circumstances he made a good start in his professional career, and his own eminent (but not *pre*-eminent) qualities have enabled him to maintain the advantage. He early in life resolved to keep his practice within £5000 a year, [perhaps it is not often that a physician has any occasion to make any such resolution,] "a limit which, while closely approaching it, he has never actually exceeded" (p. 165); and, what is much more to his credit, and what he twice mentions with creditable self-congratulation, "he has gone through a busy professional life with almost entire freedom from personal quarrel or controversy" (pp. 4, 166). Much that he tells us (and occasionally repeats more than once) about himself and his own personal habits might have been omitted without any great loss, and the space thus saved might have been allotted with advantage either to additional notices of his contemporaries, or to matters of professional or literary or philosophical interest, on each of which we should have been glad to have had more of his opinion. He gives (p. 214) an interesting account of Wollaston's last sickness, though (as we learn from another source, Wigan's 'Duality of the Mind,' p. 179) he was incapacitated by sudden illness from following up his attendance to the very end of Wollaston's life. He mentions (as he does also in his 'Medical Notes and Reflexions') the "philosophical mind of Wollaston, taking calm but careful note of its own decay—the higher faculties (which were little if at all impaired) occupied in testing, by daily experiments of his own suggestion, the changes gradually taking place in the functions of the senses, the memory, and the voluntary power." Wigan (to whom the case was especially interesting, as bearing upon his theory of the double nature of the brain) tells us that he "died from disease of one brain, producing entire disorganisation of it;" and gives "another example of his full possession of some of the inferior mental faculties, which is very remarkable. At a time when even his medical friends, Dr. Babington and others, supposed him to be so near death as to be perfectly unconscious, and when he was unable to speak, he marked down a few lines of figures on the slate, and added them up correctly, as a signal

to them that he was still quite conscious of his state, and therefore able to appreciate their services."

In reference to the chapter of his 'Medical Notes and Reflexions' "On the Brain as a Double Organ" (which procured him the honour of the dedication to him of Wigan's remarkable book), he gives a striking case of what he calls "duplicity of the will" in a gentleman who "told him that a propensity to kill Mr. Canning had come upon him suddenly," and who "eagerly besought him to protect him against himself" (p. 177). He gives an interesting account of the personal appearance and manners of Dr. Thomas Young, the only physician (as Dr. Latham says, in his 'Lectures on Clinical Medicine,' p. 9) who, from the extent and variety of his knowledge, came up to the requirements of an Introductory Lecture. "His profound and very various knowledge (says Sir Henry Holland, p. 214) was concealed under a certain spruceness of dress, demeanour, and voice, which strangely contradicted his Quaker origin, and perplexed those who had known him only from his scientific fame. I have seen the discoverer of some of the grandest and most occult laws of Light loitering with ladies in a fashionable shop of Bond Street, helping them in the choice of ribbons and other millinery. But what might hastily be deemed affectation was in Dr. Young not really such, but genuine courtesy and kindness of heart."

In what Sir Henry says of Cavendish (p. 212) whom *he appears* (though it is not so stated in express terms) to have met frequently at the Sunday evening meetings of the Fellows of the Royal Society at the house of Sir Joseph Banks, there is either some error or an ambiguity that should be cleared up in the next edition of the 'Recollections.' Cavendish died in 1810, and Dr. Holland (as he then was) did not become F.R.S. until 1816.

There are several other improvements which we hope to see carried out in a future edition, and which will render the book both easier and pleasanter to read. At present it is very badly arranged, and contains a great amount of tedious repetition, sometimes of facts and incidents, sometimes of ideas; this might be remedied without much trouble, and at the same time a Table of Contents, or an Index, or both, might be furnished, which may be considered an almost indispensable adjunct to a book containing so much interesting and unconnected matter.

[Since the above remarks were written a second and a third edition of the 'Recollections' have been published, to which a Table of Contents has been prefixed, but in other respects the book continues almost unchanged.]

IV.—The Larynx and the Laryngoscope.

THE three treatises which form the subject of this notice illustrate in a very striking manner the rapid development of our knowledge of laryngeal diseases. It is not so many years since Dr. Horace Green's work excited much interest in this country and some incredulity, by the statements it contained as to the author's method of treating affections of the throat. At that time it was regarded as barely possible to brush out the larynx with a caustic solution, or to penetrate so far as the vocal cords with an instrument.

Now we have a record of 100 consecutive cases of growths in the larynx removed by a single operator. The rapid growth of our powers of diagnosis and skill in treatment is almost equally well illustrated by a comparison of the excellent article on diseases of the larynx in the new edition of Holmes's 'System of Surgery' with that in the first edition of the same work published in 1862. The results of eight intervening years, embraced as they are in the more recent article, have entirely changed the aspect of this branch of practical medicine. Like most other modern achievements, this one has resulted not so much from any pathological discovery as from an improvement in our methods of investigation. Conditions that we could before only guess at, we can now see clearly reflected in the laryngeal mirror. The narrow chink of the *rima glottidis*, which in our days of darkness we endowed with such exquisite sensibility, that we thought it barely possible to safely traverse it with an instrument, we now boldly approach with our forceps, our knife, or the galvanic cautery. Just as our diagnosis has become more certain, so our treatment has become more scientific and more successful. To no one after the illustrious inventor of the laryngoscope is this progress more due than to the author of the works now before us. Dr. Mackenzie has given many years of steady and devoted attention to this subject; and ever since the publication of the first edition of his book on 'The

[1] 1. *The Use of the Laryngoscope.* Third edition, with several additional illustrations and new matter. By MORELL MACKENZIE, M.D. Lond., M.R.C.P., &c. &c. London, 1870.

2. *Essay on Growths in the Larynx: with Reports and an Analysis of one hundred consecutive Cases treated by other Practitioners since the Invention of the Laryngoscope.* By MORELL MACKENZIE, M.D. Lond., M.R.C.P., Physician to the Hospital for Diseases of the Throat, and to the Royal Society of Musicians; and Senior-Assistant-Physician, and Lecturer on Diseases of the Throat, at the London Hospital. London, 1871.

3. *A System of Medicine.* Edited by J. RUSSELL REYNOLDS, M.D., F.R.S. Vol. iii. London and New York, 1871. Article, "Diseases of the Larynx." By MORELL MACKENZIE, M.D.

Use of the Laryngoscope' has been recognised as our first authority. A work that has reached a third edition needs little comment, and this one is so well known that we shall merely allude to the improvements which have been introduced. These consist mainly of descriptions of new laryngoscopic apparatus, the chief of which are the author's excellent clinical lamp and examining chair. The latter looks at first sight a somewhat formidable kind of pillory for the patient, but we can speak strongly from our own observation of its practical utility. Laryngeal 'sounds' for examining the attachments of new growths, or for investigating the depth and extent of ulcers, are figured for the first time. In the section on inhalation a description is given of the author's 'eclectic inhaler,' which is certainly the most perfect instrument of the kind yet invented. The treatment of many forms of chronic laryngeal disease will be greatly facilitated by the use of this apparatus; and if we mistake not the old method of inhalation, will, now that it can be applied more perfectly, again assert its supremacy in many cases over the more recent fashion of using atomized fluids. Such are some of the chief additions in this new issue. There are many minor improvements, and the book is even more handsomely got up than its predecessors.

The essay which gained for Dr. Mackenzie the Jacksonian Prize in 1863 was at the time promised to the profession, and has ever since been eagerly expected. We can easily understand, however, that the time for publishing it in its original form has long passed. The great additions made since then to our knowledge of laryngeal diseases have compelled our author to publish a series of monographs instead of his original essay. The second of these is devoted to "Growths in the Larynx," and by its excellence fully reconciles us to an arrangement, which promises to give us a series of essays of great value.

All malignant growths have been intentionally excluded; the growths treated of are defined as "new formations of benign character, forming projections on the mucous membrane of the larynx, generally giving rise to aphonia or dysphonia, often to dyspnœa, and occasionally to dysphagia." The synonyms by which these growths are known are given afterwards, so that the scope of the work is clearly defined at the beginning. A historical sketch of the subject follows, from which we learn that Koderick in 1750 was the first to operate on a laryngeal polypus. More than a century later only nine instances had been recorded in which attempts had been made to remove the growths during life. One of these is so vaguely described, that it is of little value; in the remaining eight cases the growths were removed four times through the neck, and four times through the mouth. Such was the work of one hundred years in

pre-Laryngoscopic times. The dozen years that have followed the invention of the laryngoscope have seen some 289 cases treated, of which no less than 100 are recorded by the industrious author of this essay. We know no more convincing commentary on the vast influence exercised on our art by the invention and application of new methods of research.

To the thoughtful student, moreover, these facts are pregnant with the encouraging lesson, that when accuracy in diagnosis is once attained, art is not slow to discover new methods of alleviating suffering and saving life. Section the third is devoted to the symptoms, which are divided into functional and physical, after Dr. Causit's classification. This has been adopted in preference to the division into subjective and objective employed in the essay in Reynold's 'System of Medicine.' This chapter abounds in useful suggestions respecting the value of the first class of signs; and under the head of laryngoscopic signs there is given, after an enumeration of the more common position of the growths, a description of the appearance of each kind of polypus, according to its pathological nature. The pathology follows, and is very carefully written, and beautifully illustrated by drawings of the microscopical characters of the growths, and by exquisitely soft and finished chromo-lithographs of the laryngeal appearances. The artists deserve the highest praise for their work, which appears to have been executed under the inspiration of an enthusiasm equal to that of the author. We now pass on to the treatment. Here we are glad to find *in limine* the admission that operative procedures are not required in all cases. There is an honesty about this which some specialists would do well to copy. Small growths on the epiglottis or ventricular bands, especially fibromata, when the inconvenience is not great, may well be left alone. Sometimes the neoplasm is too ill-defined to warrant removal, and sometimes the age of the patient or the unimportant nature of the symptoms justifies non-interference. Treatment is, however, most commonly required. Little is said of palliative procedures; the radical treatment naturally occupies nearly all our author's attention. This may be (1) internal or laryngoscopic; (2) external or by direct incision into the air-passages; or (3) by the combined methods, *i. e.* tracheotomy being first performed for the patient's safety, and the growth subsequently removed through the mouth. The first of these three methods is the triumph of Laryngoscopy:

"By the removal of a growth in this way no chance of danger is incurred, little or no pain is felt, and scarcely a drop of blood is lost. By an operation of this simple character the long-lost function of a most delicate organ may be almost instantly restored, and a morbid

condition, threatening the immediate extinction of life, may be at once and for ever removed."

The continental modes of producing anæsthesia of the larynx before operation are regarded as unnecessary or useless. The plan recommended is the same as that now generally adopted before using the laryngoscope in a sensitive patient, viz., allowing a little ice to dissolve in the mouth. Sometimes a few whiffs of chloroform are useful. Three methods of removal by mechanical means are described—evulsion, crushing, and cutting. Of these the first is the most generally applicable. It is performed by means of the common laryngeal forceps or the tube forceps; whichever is used should be bent at a sharpish angle, rather than gradually curved, as in this form it does not produce irritation by pressing against the epiglottis. Cutting, which is specially suitable for some cases, is performed either by the cutting forceps, scissors, ecraseurs, or the knife. Dr. Tobold, who is no mean authority, prefers the knife. For each case there is one method most suitable, and on points of this kind the experience of the operator must be the guide. The application of caustics is, we are glad to find, not recommended, and the galvanic cautery fares likewise. Such are the various forms of the internal operation. The other operations are discussed at length, and some very important materials are collected to show their comparative merits and applicability. On this part of the subject, which is mainly surgical, we have no space to enter. In the four appendices are given full reports of 100 cases treated by the author; short notes of other cases, in which radical treatment was not adopted; next a tabular statement of the 100 cases; and, lastly, a table of all the published cases treated by other practitioners since the invention of the laryngoscope.

It is in these appendices that the author's industry and love of his work is best seen. These parts of the book render it the most valuable contribution to the pathology of the larynx which has yet appeared in any language. It is a credit to the English school of laryngoscopy.

The hospital in Golden Square, at which many of these cases were seen, has been roundly abused many times since its establishment. The publicatiou of this book, containing a record of some of the work done within the walls of that hospital, is a perfect *apologia pro vitâ suâ*. If all special institutions would only show such honest and careful work, specialism would need fewer apologists.

The third volume of Reynolds's 'System of Medicine' has part of its space devoted to chapters on diseases of the larynx, by Dr. Mackenzie. One of these is, of course, devoted to

growths in the larynx, but is by no means such an artistic production as the monograph we have just considered. It bears traces of having been written at an earlier date, and before the author's views had ripened into the full excellence displayed in his larger treatise. The other chapters on throat affections are well done, and some of them strike us as being exceptionally good, but all are too much condensed. The space allowed was evidently too limited to enable the author to do justice, either to his subject or to himself. We regret this because Dr. Mackenzie is eminently qualified to write well on diseases of the larynx. With greater space at his command, he would have made his contribution, good as it is, much better. The ill luck, to call it by no stronger term, of the 'System of Medicine' is remarkable. Every new volume brings some fresh disappointment. The authors best qualified to write find little space or none, and some who write with small authority occupy great space. If in this third volume fifty more pages, which might well have been cut off some of the other articles, had been allotted to the larynx, Dr. Reynolds would have issued a volume more fitted to meet our present needs.

V. Dr. M. Duncan on Fertility and Fecundity.[1]

Dr. Duncan's work may be described as a statistical inquiry into certain obstetrical subjects. Figures form the basis of his work, and, therefore, in the very nature of things, a wide field for controversy is opened. But it must be said that his opponents will find considerable difficulty in refuting the arguments advanced in this able work, though they may find grounds for divergence of opinion.

The work is divided into ten parts, the first of which alone is devoted to the subject from which it takes its title. This, however, is the most elaborate portion of the volume, and one with which it is most difficult to deal, either critically or analytically. It is based, as Dr. Duncan avows, almost exclusively on a considerable mass of figures. That figures may be used very differently, according to the bias of the manipulator, and that very conflicting conclusions may be drawn from the same statistical data is notorious. This volume forms no exception to that rule, though, in justice to Dr. Duncan, it must be said that he has very fairly and with great ability handled

[1] *Fecundity, Fertility and Sterility, and Allied Topics.* By J. Matthews Duncan, A.M., M.D. Second edition, revised and enlarged. Edinburgh, A. and C. Black, 1871. Pp. 498.

the materials at his command. We have some difficulty, however, in accepting all his conclusions.

With reference to the data used in Part I, it may be remarked that they comprise 16,593 children, legitimately born in Edinburgh and Glasgow in the year 1855. These figures, however, are but for one year only, and that, as Dr. Duncan shows, an exceptional one, fortunately for his purpose, as regards the mode of registration, which was only in force during the year in question. It may be urged that other years, that other towns, and, *à fortiori*, rural districts would have yielded different results. Clearly this might have been so; we have, however, to deal with the material before us.

Dr. Duncan appears to be alive to the "difficulty of handling statistics without infringement of the rules of logic," and he declares that he was all the more careful in dealing with his subject, owing to the necessity of pointing out great errors made by other authors.

Before entering upon the subject of the first part, Dr. Duncan points out that it is necessary, to avoid confusion, to establish some amount of distinction between fertility or productiveness and fecundity.

"By fertility or productiveness I mean the amount of births as distinguished from the capability to bear. By fecundity I mean the demonstrated capability to bear children."

In the first chapter the author treats upon the actual fertility of the female population as a whole at different ages. He uses Dr. Collins's statistics, showing the age of each 16,385 women delivered in the Dublin Lying-in Hospital, these women being mothers of legitimate and illegitimate children, live and dead, and his own statistics, showing the age of each of 16,301 *wives*, whose children were all born alive, and registered in Edinburgh and Glasgow in 1855.

It is necessary to point out that these two sets of statistics are not quite on the same level. It may fairly be urged in the interest of Dr. Collins's statistics, that while his gross numbers are almost identical with those of Dr. Duncan, and, therefore, equal in that respect, they are, as regards both mothers and children, more comprehensive, embracing as they do, of the former, the unmarried as well as the married, and of the latter, the dead as well as the living. However true Dr. Matthews Duncan's deductions may be as regards his own statistics, which it will be observed deal only with married mothers of living children, they can hardly be held to invalidate the more broadly based statistics of Dr. Collins, which deal with married and single mothers and with living and dead children. Therefore Dr. Duncan's conclusions cannot be held to override those of Dr.

Collins; and, as an additional argument in favour of the latter, it may be urged that the statistics of motherhood in the married cannot truly exhibit either the fertility or fecundity of females; since many women capable of bearing children are unable, for social reasons, to marry until some years after that capability has existed; and, therefore, statistics showing the fertility of both the married and unmarried are much more likely to display the true fertility of women than those which are confined to the married.

After all, Dr. Duncan agrees with Dr. Collins that most children are born of women at or near the age of thirty years; and further, that more children are borne by women under thirty than by those above that age; but they differ as to the proportion, Dr. Duncan showing it to be, according to his own data, about three fifths, while Dr. Collins, from his data, calculates it to amount to three fourths.

To the reasoning contained in Chap. II we are constrained to take exception. In it the author proceeds to compute the "Comparative Fertility of the Female Population as a Whole at different Ages," on data which are obviously insufficient, not to say fallacious. To arrive at the desired result, Dr. Duncan computes the comparative fertility of the whole female population of Edinburgh and Glasgow in 1861 by the standard of the married mothers of living children born in Edinburgh and Glasgow in 1855. In such a process as this assumptions have largely to be made, and to such a proceeding in a matter dealing with statistical data we really must demur.

Nothing should be assumed, nothing taken for granted in such questions; the figures alone should be dealt with—and these should in every possible respect correspond—as forming unalterable data for the discussion and decision of the points involved. Moreover, as already said, the figures should be for corresponding periods, else vitiating assumptions have to be made, and these, once admitted, invalidate the whole of the work.

The third chapter deals with "The Comparative Fecundity of the whole Wives in our Population at different Ages." To this chapter the objections advanced against the last do not apply, the comparison being, not of wives-mothers with women living, but of wives-mothers with wives. The first table in this chapter, however (Table VII), seems to disprove the statement made by Dr. Duncan, in the concluding sentence of the previous chapter, viz., that the fertility of the whole female population, at different ages, is greater in the decade following the climax of thirty years than in the decade preceding the age of thirty; for we see in this table that half the wives (50·00 per cent.)

between the ages of 15 and 19 bear children, 41·79 per cent. between the ages of 20 and 24, 34·64 per cent. between the ages of 25 and 29, 26·56 per cent. between the ages of 30 and 34, 20·39 per cent. between the ages of 35 and 39, 8·04 per cent. between the ages of 40 and 44, and 1·27 per cent. between the ages of 45 and 49.

It is clear, therefore, that both the fertility and the fecundity of wives-mothers under thirty greatly exceed that of the wives-mothers over thirty. This Dr. Duncan sees and admits in the following terms:—"The wives under thirty years of age were much more than twice as fecund as the wives above thirty years of age." This admission agrees with the statement at page 21. "So far as they (the data) go, they indicate a great fecundity of a mass of wives at 17, 18, and 19."

There is some difficulty, however, in reconciling these statements with one at page 17, to the effect that the comparative fertility of our whole female population, at different ages, is greater in the decade of years following the climax of about thirty years of age than in the decade of years preceding the climax. This would tend to show that fertility and fecundity are opposed to each other, which cannot be.

Chap. IV deals with the "Initial Fecundity of Women at Different Ages." This is a somewhat difficult subject to work, possible immaturity in some of the younger wives—and here Dr. Duncan deals only with the married—having to be allowed for; but from the data used Dr. Duncan concludes that the climax of initial fecundity is probably about the age of twenty-five years.

In the next chapter the "Fecundity of Women at Different Ages" is discussed, and here again Table XIV shows the percentage of *fertile* wives to be almost 100 per cent. between the ages of fifteen and twenty-four, being greatest between the twentieth and twenty-fourth years of age. According to Dr. Duncan's own tables, then, both fertility and fecundity would seem to be, as was remarked above, greater under than above thirty years of age. As bearing upon this subject, Dr. Duncan makes an interesting and valuable quotation from a work by Mr. Geyelin, C.E., on "Poultry-breeding in a Commercial Point of View," which he introduces with the remark that "a remarkable illustration of the variation of fecundity at different ages is acquired by observation of the fertility of the domestic fowl." Mr. Geyelin says that a hen cannot lay more than 600 eggs, the ovarium containing no more than that number of ovula. These are distributed over nine years, in a proportion which shows that by far the greatest share of them is laid before the commencent of the fifth year; and it follows, therefore, that

it would not be profitable to keep hens after their fourth year. Such evidence as is forthcoming tends to show that the best time for the procreation of strong and healthy children is, in women, from the age of about twenty-three to about thirty-three years.

Dr. Matthews Duncan candidly admits that this portion of his work has been adversely criticised, and that his friend Professor Tait's notes are not exactly consistent with his own views.

In Part II the author deals with the weight and length of the newly-born child, which subject he introduces with the following statement:

"Inquiring into the influence of the age of the mother upon fecundity, I desired to find out if any light could be thrown upon the subject by the variations, if any, of the weight and length of mature children born of women at different ages; intending to assume that the weight and length of the child might increase or diminish with the high or low state of the fecundity of women, or of the vigour of the generative functions."

Dr. Duncan's observations are based on 2070 pregnancies, yielding 2087 children, occurring in the practice of the Edinburgh Maternity Charity. Hecker's observations show that the weight of the children of multiparæ is slightly, though distinctly, in excess of that of the children of primiparæ, and Dr. Duncan's observations are, so far as they go, corroborative; but the latter also show what Hecker, from ignorance of the fact, did not, that the age of the mother, whether primiparous or not, influences the weight of her offspring. This is in accordance with what is known about cattle breeding; and those who are acquainted with this subject are aware that it is a matter of belief among breeders that a young heifer will not bear so fine a calf as a maturer beast. Dr. Duncan's tables indicate that mothers in the prime of life bear the heaviest children, and he thinks, rightly no doubt, that it is the maturer age of the mother, and not mere multiparity, to which this is to be attributed. But beyond the prime of life—say over the thirty-fifth year—it is thought that the primiparous woman bears a smaller child than she would have done had she been pregnant some six or eight years sooner, and that this accounts for the easier labours of old primiparæ than of primiparæ in the prime of life.

As regards the influence of primo-geniture on the length of the newly-born child, the author says that no notable difference is made out between the offspring of primiparæ and multiparæ; but, as far as the evidence goes, it is in favour of the former, whose children are a little longer than those of the latter.

Weight, however, it is added, may be a surer test than measurement: "Length does not seem to be under any law connected with the first or subsequent occurrence of pregnancy." Dr. Duncan thinks, indeed, that a larger number of observations would show comparative shortness of first-born children, "just as comparative lightness has been shown, and for the same reason, namely, because primiparous women are, in a very large proportion, young." This may be a fair inference: for the present, however, it lacks demonstration.

Table XXI shows that as the heaviest children are born of women from 25 to 29 years of age, so are the longest; the excess of length, however, is but slight. Fresh observations by Hecker on a large number of women confirm Dr. Duncan's conclusions that about the ages of 25 to 29 years women bear children whose weight and length are greater than in the periods before and after these years. Here Dr. Duncan remarks that pullets and old hens lay small eggs, and he states that the eggs of the latter are sometimes yolkless, or contain only an imperfect yolk. This again is in unison with the experience of cattle breeders, who consider the offspring of young heifers and old cows to be inferior in size and stamina to that of cows in their prime. The relation of the size of offspring to the duration of pregnancy is an interesting question, on which remarks will be found hereafter; but evidence on the subject is wanting.

In discussing the production of twins the author quotes from a valuable paper by Dr. Arthur Mitchell, who states that "the whole history of twinning is exceptional, indicates imperfect development and feeble organisation in the product, and leads us to regard twinning in the human species as a departure from the physiological rule, and therefore injurious to all concerned." Among Dr. Mitchell's conclusions are the following, which are sufficiently serious without dwelling on the curious relation of malformation to twinning touched upon by Simpson. Dr. Mitchell concludes that—

"Among imbeciles and idiots a much larger proportion is actually found to be twin-born than among the general community. Among the relatives of idiots twinning is also found to be very frequent. In families, when twinning is frequent, bodily deformities (of defect and of excess) likewise occur with frequency."

A collection of 1512 cases of twins shows that in the general population young and old mothers bear two or more children but seldom, plural births being most frequent in women between the ages of 25 and 29. Dr. Duncan thinks his own statistics prove that the age of mothers bearing twin children averages more than this; and, he remarks, that a table produced by adding Dr. Collins's data to his own, gives "the very remark-

able result that, speaking generally, the older a mother is the more likely she is to have twins." This holds good up to the age of 40. Moreover, the proportional frequency of twins increases with the number of the pregnancy up to the ninth. It should, however, be mentioned that the first pregnancy is an exception to this rule, for there seems to be an increased chance of bearing twins in the first pregnancy. As far as the evidence goes, twin-bearers appear to be more prolific than the uniparous. Dr. Duncan just touches, but does not discuss the question whether pluriparity is "the result of some transcendental primordial energies in the ovary." This is a point upon which we should be glad to hear more from Dr. Duncan.

Passing by some chapters of less general interest a pause may be made to note the author's conclusions that a fertile woman, living in wedlock, may be expected to bear fifteen children during the time she is susceptible of childbearing—from the fifteenth to the forty-fifth year. This is equivalent to a regular interval of two years between each delivery, and probably this is not far from the truth. It must not be forgotten, however, that abortions and miscarriages may occur in the intervals, and that such pregnancies may in very healthy and well-conditioned women run on to perfection instead of falling short of it, thus giving an excess of living children over others, and in this way very large families may be accounted for.

In Chapter XIV of Part IV, Dr. Duncan adduces evidence and argument to prove that although wives from 15 to 19 years of age "have, in virtue of their great perseverance of fertility, a greater total fertility than wives of any other age," wives from 20 to 24 years of age are most prolific in desirable offspring, and contribute most to the adult population. The marriages of the former are looked upon as premature, owing to the immaturity of the women, which exposes them to many evils. We glean that a woman married say at 23 or 24, is likely to bear as many *surviving* children—or children who will attain adult age—as one who marries earlier; and the former will have a great advantage over the latter in the protection afforded her by her greater maturity against certain perils of childbed.

There appears to be great difficulty in rightly ascertaining the "Comparative Fertility and Fecundity of Different Peoples;" the necessary data being as yet wanting. This is a subject of great interest and of no small importance, comprehending as it does the great questions in political economy regarding population, and the various means of increasing it, or of retarding its excessive growth. The doctrines of Malthus and his disciples on the one hand, and of his opponents on the other, are involved in the matters discussed in this part of the work. Dr. Duncan's

cry is for more light, and in the interests of science it is to be regretted that he is not furnished with it.

As regards the "Sterility of Wives" the data at command, which are but scanty, show that one in every eighty-five wives is sterile. This statement, however, is perhaps scarcely to be relied on, owing to the insufficiency of the data on which it is based. The age of a wife has a material influence on her sterility or the reverse. Thus, while only a small per centage of wives between the ages of 15 and 19 are sterile, sterility practically reaches its vanishing point between the years 20 and 24, rising again rapidly after this.

On the "Expectation of Sterility" Dr. Duncan says: "The main element in the expectation of sterility is the age of the woman at marriage." Statistics suggest two laws which are thus put:— "Of these the first is, that the question of a woman being probably sterile is decided in three years of married life; the second, that when the expectation of fertility is greatest, the question of probable sterility is soonest decided, and *vice versâ*."

"Relative Sterility" is the term applied only to those who have borne children. As the author remarks, "all these wives, if they survive in wedlock, will sooner or later become relatively sterile." The conclusion he arrives at is that "relative sterility will arrive after a shorter time (not earlier), according as the age at marriage is greater." But he quotes in the context a table of Professor Tait's, which shows that "the older a fertile woman is at marriage, the older is she before her fertility is exhausted, that is, before the advent of relative sterility." As to the advent of sterility, Dr. Duncan states the following law: "A wife who, having had children, has ceased for three years to exhibit fertility, has probably become relatively sterile, that is, will probably bear no more children; and the probability increases as time elapses." He aptly remarks that the conclusions arrived at in the part of his work devoted to the subject of sterility, will afford a means for estimating the utility of methods for curing sterility, a class of practice which he appears to regard with suspicion.

Part VI consists of a learned, able, and exhaustive "Note on Formulæ representing the Fecundity and Fertility of Women" by Professor Tait, who worked out the subject for Dr. Duncan, and a reprint of a critical article (which Dr. Duncan justly characterises as "ingenious and valuable") from the 'North British Review' on the first edition of the work. Into this part of the work we do not propose to enter. Suffice it to say that Professor Tait's elaborate calculations, in the main, support the conclusions arrived at by Dr. Duncan; while the review, a very

able one, is highly commendatory of the work, though it takes exception to some of Dr. Duncan's views and inferences.

The "Mortality of Childbed" has long been a cause of warfare, the echoes of which have not yet died away, though one of the leaders in the strife, Sir J. Simpson, has dropped out of the fray.

In discussing the subject the author explains at the outset the sense in which he uses the term "mortality of childbed." He says:

"Childbed deaths include those from childbirth and metria. Mortality, or deaths *of* childbed, are those belonging to that state, *i. e.* childbirth and metria deaths. Mortality, or deaths *in* not *of* childbed, include all deaths in the four weeks of childbed. Deaths *in* not *of* childbed, are all deaths, deaths from whatever cause, occurring within the four childbed weeks, including the period of labour."

Disclaiming the wish to discuss doubtful points, averring that at present we are not possessed of data which would enable us to ascertain in anything like a satisfactory manner what the mortality of childbed is, Dr. Duncan declares that his aim is to contribute to the acquisition of more precise knowledge than we at present possess on the subject. A great and fundamental difficulty is encountered at the outset; viz.:—that the facts themselves are in dispute—there is no agreement as to what constitutes a childbed death.

Notwithstanding this, much valuable knowledge is obtainable, for although we cannot get to know the deaths *of* childbed, we can those *in* childbed, and the two, Dr. Duncan thinks, are not very different. The memorable discussion in the Dublin Obstetrical Society is much used by Dr. Duncan, who quotes largely from the able speech of Dr. M'Clintock. This gentleman, as is well known, dwelt forcibly on the fact that while in lying-in hospitals all deaths, from whatever cause, occurring in such institutions are recorded and debited against them, many of the deaths occurring in puerperal women out of doors, even though due to puerperal causes, are not so registered; and thus a fallacy is introduced utterly vitiating all conclusions based on such data.

Derived from any source, the data will, Dr. Duncan thinks, be imperfect in many ways. He is of opinion that the best and most trustworthy are those afforded by well-kept hospital records, and by the reports of Registrars-general. With report to the latter, Dr. Duncan implies that something more than the mere figures they afford is needed to prove what they appear to show, viz., that the mortality of childbed has in England and Wales been reduced from 1 in 167 in 1847 to 1 in 226 in 1856.

He scouts the idea that this diminution in the mortality of childbed is due to the progress of midwifery. That this may be so we are not in a position either to affirm or to deny. But surely Dr. Duncan must be oblivious of the progress of scientific midwifery—difficult as such a supposition is in the case of one who has such high claims to be considered one of the ablest living exponents of modern obstetrical science as he undoubtedly has—to say nothing of the advance of medical knowledge generally and of hygiene in particular, when he allows himself to pen such a sentence as this: "For my part I think obstetrical common sense will be very contented if the true childbed mortality of London is at all less than in the London of 1660"! Does he suppose that obstetrical science has remained dormant in London since 1660, and that its practitioners in the present day are as ignorant as the midwives of that day? Have the forceps done nothing? has chloroform done nothing to lessen the mortality of lying-in women since 1660? Surely these two agents alone, to say nothing of other obstetrical aids of recent invention, must be credited with some power to influence the mortality attending childhood? It is obvious that here Dr. Duncan's position is untenable.

Dr. Duncan throws aside as worthless the data furnished by private and dispensary practice. He severely handles Le Fort's statistics, which, he says, "show how bad they (maternity hospitals) may be, and nothing more." Le Fort's statistics represent the mortality in hospitals or maternities to be 1 in 29. "Such statements," says Duncan, "regarding the mortality in hospitals only show how disgracefully mismanaged many hospitals are, how much need there is of the exertions of the philanthropist." Having made out the mortality of hospitals to be 1 in 29, Le Fort seeks to demonstrate that in home practice it is but 1 in 595, whereon Dr. Duncan asks, "Is any one so foolish as to believe it?"

Proceeding to the results afforded by the various sources of information available, Dr. Duncan begins with hospitals, some of which show a disgraceful mortality, 1 in 3, while others exhibit a mortality which is, according to the author, nearly that of the present ordinary mortality of childbed. He deals chiefly with the Golconda of obstetrical statisticians, the Dublin Lying-in Hospital, which presents a mortality of 1 in 95, taking all the figures for seventeen years. The data afforded by different Registrars-general he distrusts, the variations being so great; and, for the same reason, he doubts the correctness of the figures obtained by a private search of the public records and the records of private practice. The general result Dr. Duncan arrives at is, that "Not fewer than 1 in every 120 women delivered at or

near the full term die within the four weeks of childbed." We would fain hope that this estimate is excessive, but that it has a show of approximation to the truth is, we fear, borne out by a remark of Dr. Duncan's, that "Simpson has said that the mortality of childbed is now 1 in 150 or 200" ('Obstetric Works,' vol. ii, p. 482).

A dispassionate consideration of the whole subject leads to the conviction that the mortality in lying-in hospitals, as at present constituted, is decidedly greater than that in out-patient practice. Upon this point there is general agreement: the question in dispute is whether such a high rate of mortality is a necessary concomitant of hospital delivery; whether in fact if hospitals were in every respect well managed they would yield such a regrettable harvest of death; whether the death rate should be any, if at all, higher than that of out-patient practice. This is the problem which demands solution; meanwhile the sad fact remains that, other things being equal, the chances a lying-in woman delivered in hospital has of passing safely through the perils of childbirth are appreciably less than those of a woman delivered at her own home.

The "Relation of the Number of the Labour to the Mortality from Puerperal Fever" is next considered. On this Dr. Duncan remarks that there are two questions interesting and important, viz.: "Does the number of a woman's pregnancy indicate in any degree the mortality to be expected from lying-in?" and "Does the age of the child-bearing woman indicate in any degree the mortality accompanying this function?"

The consideration of this subject is hampered by the fact that "a large amount of puerperal mortality is produced by that indefinite class of diseases unphilosophically and injuriously combined under the name of puerperal fever." Hereon Dr. Duncan makes some weighty remarks. While it is not his intention to enter on the vexed questions in reference to this erroneously called fever, statistics may, he thinks, suffice to demolish some of the errors attaching to it. "The invasion of this disease is well known to be described by a class of obstetricians as an 'accident.' To remove it from this category is an object of just ambition. To some extent this has already been done by Sir J. Y. Simpson, who has shown that it is subject to the law of the duration of labour. The object will be further promoted if it can be shown to be under a law of the number of pregnancy, or of the age of the mother, or of both."

The author proves that the deaths from puerperal fever among primiparæ are twice as numerous as among pluriparæ. Hugenberger's data show that after the first labour there is a comparative freedom until the fifth pregnancy is reached, when

the mortality begins to rise again, increasing with the pregnancies until it reaches a degree as great as that of the first confinement.

The "Relation of the Number of the Labour to the Mortality Accompanying Parturition is next discussed." Here again it is shown that purely natural first labours are about twice as fatal as purely natural subsequent labours; but when not purely natural there is but a slight difference between the mortality of first and subsequent labours; the relative mortalities are in fact nearly alike.

The somewhat unsatisfactory data at Dr. Duncan's command for determining the comparative per centage of deaths in successive pregnancies after the first, show that after the ninth labour the risk increases greatly. On this he remarks, "The law of duration of labour, then, does not enable us to explain the variations of mortality in different labours." Still, he rightly attributes much importance to the duration of labour, though not to the mere addition of length to a labour; the introduction of complications forms the main explanation of the law of the duration of labour as pointed out by Sir James Simpson, and without these complications duration would be of small importance, in accordance with the opinion generally held.

There is abundant evidence to prove that primiparity exercises great influence on the mortality of child-bearing women. It largely accounts for the fact that the mortality of married females is higher than the mortality of unmarried females below thirty years of age. Above this age the balance is the other way; and Dr. Stark argues that the explanation of this is the additional danger in the birth of the first child. Both he and Dr. Farr declare that marriage exerts a favorable influence on the mortality of women; but to this Dr. Duncan objects that married women are picked lives, they being presumably healthy and well formed, while the unmarried include the opposite class—the sickly and deformed, whose vitality is low.

As regards the "Relation of Age to the Mortality from Puerperal Fever," Dr. Duncan quotes from an important letter by Dr. Farr, who, taking the Swedish returns, shows that the mortality from puerperal fever is greater in women under twenty-five than between that age and thirty-five, above which it rises again, though not with the rapidity or to the extent that it does in the years fifteen to twenty-five. Here again, however, primiparity exerts a disturbing influence.

The "Relation of the Age of the Mother to the Mortality Accompanying Parturition" is next considered; and again using Dr. Farr's data, the author shows that women aged from twenty-five to thirty-four, have the fewest deaths among them; then

come those aged from fifteen to twenty-four, who exhibit an increased mortality, slight, however, when compared with that of women over thirty-four, in whom the mortality advances in a far higher ratio. From other data, however, Dr. Duncan concludes that the age of least mortality is near the twenty-fifth year, and that on either side of this period mortality gradually increases with the diminution or increase of age; the increase being at a much greater rate above than below this age, decidedly throwing the greater safety to the side of the quinquenniad, twenty to twenty-five. He justly remarks that "it is too interesting to escape notice, that the age of greatest safety in parturition coincides with the age of greatest fecundity, and that during the whole of child-bearing life safety in parturition appears to be directly as fecundity, and *vice versâ*."

In discussing the vexed question of "Puerperal Fever in Hospital and in Private Practice," the author at the outset attacks the definition, or rather the absence of definition, of what is puerperal fever. He says, "Puerperal fever or metria is to me a hot-bed of insufficient hypotheses. I do not believe there is any such disease. The term includes a variety of diseases and a variety of modifications or terminations of disease." He believes there never was an epidemic of puerperal fever, and that authors who speak of such occurrences have never defined what they mean by puerperal fever. He is sceptical as to the preventibility of the disease, and says there is a regular and practically constant mortality from it, of which both doctors and people seem ignorant. As to its supposed greater frequency in lying-in hospitals than in private practice he is also sceptical, and to justify his disbelief adduces tables showing that there is from this cause a constant mortality, which is but slightly greater in hospitals than in private practice, and this excess he attributes "to the general degradation, to the comparatively great number of the seduced, and to other unfavorable conditions among those delivered in hospitals." When deaths from metria are greater in hospitals than in private practice, he declares that it is owing to bad arrangements in the former. Lastly, he says, "I can find no ground for the awful suspicion that well-managed hospitals have caused a large, unnecessary, or avoidable mortality, or developed diseases previously unheard of."

With reference to "Aggregation as a Source of Danger to Lying-in Women," which has been maintained in the affirmative by Stark and Simpson, who asserted that the mere congregation of human beings in one building is a source of danger to their lives, all the greater if they are sick, Dr. Duncan says, "In regard of all these assertions, I unhesitatingly express my assurance that they are without any sufficient foundation." He argues forcibly

against the "mere aggregation" hypothesis. He admits that, "as you leave the rural districts, and pass through different degrees of aggregation on to overcrowding in an hospital, you have increasing and new sources of insalubrity; but these causes of insalubrity may be counteracted; they are not inevitable nor invincible." He quotes with force the "gigantic fact" of London, which is often said to be the healthiest town in the kingdom, and which certainly has a smaller rate of mortality than many villages. The salubrity of London is astonishing. The registrar of one of the districts in St. Marylebone claims that it is the healthiest in the kingdom, although it includes within its area many small streets and courts. The fact that the greater part of this subdistrict has a gravelly subsoil deserves mention.

In support of this view, that mere aggregation, even in hospitals, is not *per se* a cause of mortality, the author quotes a table from the well-known work of Dr. Evory Kennedy, from which he draws conclusions the very opposite of those deduced by Dr. Kennedy, thus illustrating anew, and forcibly, the the truth of the saying that "figures can be made to prove anything." Dr. Duncan's arrangement of the data afforded by the Dublin Hospital shows, as he says, "without difficulty or strain," that the mortality of that hospital does not increase with the increased number of the inmates. The mortality of that hospital is neither in the direct nor in the inverse ratio of the aggregation. "This is a practical result, for it sets in-inquiry into other directions to find out the hidden sources of increased mortality."

In like manner the statistics of the Vienna and Paris lying-in hospitals are used to show the same general result, and finally the author asserts that the baneful influence of aggregation has not been proven.

The "Age of Nubility," Dr. Duncan justly argues, ought not, either on physiological or moral grounds, to be assumed to be coincident with the advent of menstruation. This is clear enough, for no one would advocate the marriage of a girl of twelve, however normally she may menstruate; and, on the other hand, in the absence of menstruation and other signs of normal development, no one would advocate the marriage of an immature girl, even though she may have attained the age of eighteen, or even twenty. A number of reasons support the view, that in the interests of parents children and society, it is desirable that the marriage of girls should be postponed until they are mature enough, not only to bear children, but to bear such as have the best chance of surviving; not only to bear the

risks of childbirth, but with the best chance that they themselves will safely go through its perils.

The author says his object is to indicate what age is the wisest for a woman to enter the married state. MM. Dubois and Pajot judiciously remark that "Nubility not only implies the abstract faculty of procreating, but the possibility of a procreation not injurious to the mother or to the infant." Joulin justly says, "Nubility is the complement of puberty. These two states should not be confounded." Joulin also remarks, with great force and practical common sense, that, as a rule, a girl should not be married, that is, exposed to the chance of becoming a mother, until for a year, at least, her stature has ceased to increase.

This is a sensible and practical rule where it is so difficult to draw the line. Dr. Duncan shows that nubility implies and demands a certain amount of physical development; the full stature—quoad the individual—should have been attained, the pelvis should be fully developed in *shape*, and the genital organs should also have attained their full development. He quotes Arnold ("Handbuch der Anatomie des Menschen") to show that the dimensions of the womb before and after the twentieth year are materially different: a statement that is supported by Klob and Kussmaul. Quetelet indeed thinks women generally continue increasing in stature until the twenty-fifth year. Dupuytren, Burn, and Mr. John Wood all agree that the pelvis undergoes great changes in shape at and after puberty; the transverse diameter beginning greatly to exceed the conjugate, a change which is not completed until after the eighteenth year. Dr. Duncan quotes a remark by that wonderful man Aristotle, whom nothing seems to have escaped, that "premature wedlock is peculiarly dangerous to the female sex." Litzmann's observations are confirmatory of the immaturity of the pelvic development of girls of eighteen or nineteen. Duncan and Hecker show that the length and weight of firstborn children are below the standard of other children; and Dr. Arthur Mitchell has shown that immature and old mothers are specially liable to bear idiot children. Another argument against early marriages is the fact established by investigations that families over ten in number are generally borne by women who have married early. The arrest of development produced by over-early breeding is seen both in women and in the lower animals. As regards the latter, the knowledge is as old as Virgil at least.

The result of Dr. Duncan's own reflections on this question of nubility is, that as a rule women should not marry under twenty. This in fact is the logical outcome of all the able reasoning the work contains; and it is one that must command the

willing assent of every competent thinker, and certainly deserves to be more widely known and acted upon than we fear it is likely for some time to be. Its recognition is sorely needed; its influence on the common weal would be extremely beneficial, for who can doubt that as education advances and sinks deeper into and permeates the lower strata of society, such knowledge would exercise a deterrent effect on the class from which over-early marriages are mainly recruited; the artisan class, whose lads early in life can earn wages which soon put them into a position to marry. This is a matter of deep interest to the State, whose most valuable wealth consists in a healthy and vigorous population. Dr. Duncan has with great ability shown by what influence this may be affected; and inferentially, how it may be secured. The spread of snch knowledge might reasonably be reflected to materially strengthen the position of the country; for a healthier population would make it wealthier, and a more vigorous population would increase its strength. The author's able researches strike at the root of the whole matter. He shows what is the best time for breeding, and, as breeding must go on, this is no slight matter. In the face of increasing urban life it is one of growing importance. The fallen fortunes of France might be cited in this connection, but we refrain out of respect for its miseries. True, Dr. Duncan only deals with the mothers, and has nothing to say about the fathers; but Darwin has shown that the female exerts most influence on the race, and is presumably the most important parent of the two.

It should be the business of sanitarians, in this country at any rate, not only to look after the hygienic environments of the individual when he is born, but as far as may be practicable to secure, by education and by the spread of sound physiological doctrine, such as is here inculcated by Dr. Duncan, his healthy procreation. Who doubts that the more vigorous a nation is the better it is? Do not the records of bygone ages tell us again and again that vice, begotten of effeminacy, has always been the forerunner of decay? Shall the warnings and example of ancient Rome fall on us unheeded, as did the ruin of Byzantium on the Romans. It is necessary to the well-being of every country that its children be strong and capable, and neglect or failure in the production of a vigorous race is sure to end in its destruction.

Passing from this great subject, however, we note that Dr. Duncan justly attributes to the duration of labour a high place in the science of midwifery. He asserts with considerable force that the use of statistics in the discussion of the debated questions has been such as to render it difficult to decide whether their application has tended more to elucidate or confuse. It

is the battle of figures over again. He puts the whole matter of the duration of labour very pithily when he says, "The dangers of child-bearing are for the most part concentrated into the period of labour, or derive from it their origin. The longer the labour there will be the more opportunities for such dangers to intervene; and hence it naturally follows that the mortality of women in parturition and childbed increases with the increasing duration of labour." This proposition has been proved by Simpson's and Collins's statistics. The method of having formally enunciated this belongs, Dr. Duncan says, to Sir J. Simpson. Mere length of labour Dr. Duncan does not admit to be a cause of increased mortality; it is that which long labour implies that he regards as the cause of the mortality. But, *per contra*, Dr. Duncan calls attention to the generally received opinion that very rapid labours are, comparatively speaking, injurious and dangerous. Tarnier also quotes Hippocrates to this fact, which is generally admitted.

In discussing another proposition, viz., "The duration of labour, is only an inconsiderable part of the many causes (single or combined) of the mortality of women in parturition and the subsequent childbed," Dr. Duncan says that the mere duration of labour, considered in itself, and apart from other causes of danger likely to spring up as the process becomes protracted, is of little consequence, so far as the recovery of the mother is concerned. This doctrine, which is deeply impressed on all the valuable literature of the profession, is one which he endeavours to defend and confirm, for it places him in direct collision with Sir J. Simpson, who held and taught the very opposite. In support of his own views Dr. Duncan quotes Dr. Collins on the same side, who, however, makes the damaging admission that the mortality from protraction of labour, apart from other causes, is comparatively small. True, but according to this it is some, and if it be ever so little it proves that protraction of labour, apart from other causes, is a cause of mortality in parturition. It may with truth be said that long labour involves more or less exhaustion, renders the soft tissues engaged more prone to mischief, and generally puts the patient in a worse position to undergo the fatigue and effort of actual delivery, and to combat the perils of the subsequent puerperal state. But here any hard and fast rule derived from statistics or otherwise is misleading and mischievous. Women differ very widely in their power to withstand long-continued labour, and the obstetrician should beware of allowing himself to be over-much influenced by the view that length of labour is of no consequence. On the other hand, uncalled-for interference with the course of labour is much to be deprecated; the evidence calling

for special aid should be clear and distinct—none but the unskilled would expect or wish it to be glaringly palpable—and then action should be prompt. The question of the influence of protracted labour on the life of the fœtus, which all admit is jeopardised in proportion to the length of the labour, cannot altogether be put aside in discussing the duration of labours as a cause of maternal mortality, for the death of the child is not unfrequently a source of great distress and depression to the mother; it is consequently a circumstance capable of exercising considerable influence on her recovery.

The duration of pregnancy has for ages been a subject of deep interest and keen dispute. It is not a little singular that to this day it remains unsettled, even with the aid of information obtainable from the lower animals. Duncan dwells on the error which many writers on the subject have fallen into of confounding insemination with conception. Clearly, conception occurs only when the semen unites with the ovum; but the determination of the exact time of this conjunction is a work of surpassing difficulty. The interval elapsing between the deposit of semen within the female genitals and its reaching the ovum varies considerably, and so though the time of insemination may be known, that of conception can only be guessed at. The impregnating power of semen depends on the life of the spermatozoa; these, in suitable conditions, retain their vitality for a considerable period, for days in mammals, and, according to Valentin, in some insects for months. Semen, therefore, may remain in the genital passages of the female for days before it comes in contact with the ovum, before, in other words, conception takes place; and therefore this unknown period must be deducted from the time elapsing between coitus and parturition, to enable us to arrive exactly at the duration of pregnancy.

The interval between insemination and parturition is of greater practical importance. Its medico-legal bearings are self-evident. Duncan, from statistics derived from Montgomery, Raciborski, Rigby, Reid, Veit, Hecker, and others, showing the results of a single coitus, or of intercourse extending over two days at most, arrives at the conclusion that the average interval between insemination and parturition is 275 days. This, like all statements resulting from merely enumerative induction, must be regarded as temporary and liable to correction; still, no doubt it is a pretty exact expression of the truth. The interval between the last menstruation and parturition widens the gap, but in obstetric calculations is all important. "In the vast majority of cases," as Duncan truly says, "it is the only fixed point from which the calculation can be made." What we know about the descent of the ovum is corroborative of the

general belief that the most favorable time for conception is a few days after menstruation, and this view is supported by many physiological considerations.

"Prediction of the Day of Confinement" is a subject Dr. Duncan may almost be said to have made his own. We are, indeed, acquainted with no writer who has done so much to improve our knowledge on a matter so useful to accoucheurs. The information he gives may be put thus: No safe prediction of the day of confinement can be made; the probable day of confinement may be stated with some exactness; it is likely to be 278 days after the day of disappearance of the menses; in other words, nine months and three days after the last day of the last catamenial period, or, roughly speaking, ten lunar months. This has a practical bearing other than that of furnishing data for the prediction of the day of confinement. It affords a valuable guide to the induction of premature labour, which, according to physiological indications, should be brought on only at a time which but for pregnancy would have been a catamenial period. No one can deal justly with this subject without referring to and acknowledging, in terms of the highest praise, the philosophical observations of Dr. Tyler Smith on the duration of pregnancy and on the cause of labour. That able physician has shown that ovarian influence is the determining cause of labour as it is of menstruation, and that delivery may be expected on some day of what would have been the tenth catamenial period had not pregnancy intervened. Dr. Clay, of Manchester, argues that the variable duration of pregnancy may be influenced by the age of the parents, especially that of the mother, the younger the parent the shorter being the term of gestation. The well-known observations of Lord Spencer on cattle show that gestation with males has a tendency to continue a few days longer than gestation with females, and that cows impregnated by a certain bull went a few days longer than when impregnated by other bulls. These facts should be borne in mind in predicting the day of confinement. Dr. Duncan gives a chapter on "Practice in the Prediction of the day of Confinement," in which he embodies the experience derived from 153 observations, in which he privately predicted the day of confinement in his note-book, with the following results:—In ten cases the day was exactly predicted; in eighty confinement took place sooner than was predicted by an average of seven days; in sixty-three confinement was later than predicted by an average of eight days. However, in sixty-three cases the prediction was pretty successful, though not exact, for delivery took place not earlier nor later than 4 days from the predicted day. His experience leads him to advise the pre-

diction, not of a day, except for private use, but of a fortnight. Practically, though the calculation is purely empirical it is found best to count from the end of the last menstruation, and the 278th day after this, Dr. Duncan says, is the average day of delivery at full term.

Protraction of the period of pregnancy has always been a subject of great difficulty and has given rise to much discussion, but no explanation of its cause has yet been propounded which commands general acquiescence. It is a subject exceedingly difficult of investigation, owing mainly to the almost impossibility of obtaining trustworthy data. There seems, however, to be pretty satisfactory evidence that the advent of pregnancy may be considerably protracted, but why this occurs we do not at present know. The most philosophical explanation of the occurrence would appear to be that which attributes impregnation to a fertilizing coitus just upon the period of menstruation, which it thus anticipates and prevents. Evidence is not wanting in support of this view, which also appears to be the most reasonable as well as probable explanation of the majority at least of the supposed cases of undue protraction of the period of pregnancy.

This hypothesis, however, will not account for or explain all the cases; and, doubtless, there are instances of real protraction. In these Dr. Duncan argues that corresponding development of the fœtus should be present, as indeed was the case in some examples quoted; but on the other hand there were instances in which the fœtuses were remarkable for their smallness. Mention must not be omitted in this connection of the views of Dr. Tyler Smith already referred to, that the time of parturition is a multiple of the catamenial periods, and that, as in some women this normally exceeds a month, so the duration of pregnancy may in like manner in such persons exceed the ordinary period of gestation.

The work concludes with a comment on the opinions of Harvey and Dr. Montgomery on the same subject. While the latter is held by Dr. Duncan to make the period unduly long, the sage observations of the immortal discoverer of the circulation of the blood are shown to be in exact accordance with the latest scientific investigations, which give a period of 278 days as the ordinary duration of pregnancy. The following are the piously expressed remarks of Harvey on this subject:—

"Unquestionably the ordinary term of utero-gestation is that which we believe was kept, in the womb of his mother, by our Saviour Christ, of men the most perfect; counting, viz., from the festival of the Annunciation in the month of March to the day of

the blessed Nativity, which we celebrate in December. Prudent matrons calculating after this rule, as long as they note the day of the month in which the catamenia usually disappear, are rarely out of their reckoning; but after ten lunar months have elapsed, fall into labour and reap the fruit of their womb the very day on which the catamenia would have appeared had impregnation not taken place."

It remains for us to say of Dr. Duncan's very able work that it is scarcely possible to do justice to it in a review. It is too profound for summary judgment. To every one who studies midwifery scientifically it is essential; and to those who deal with one of the most important social problems of the day, viz., that of the laws of population, its perusal will afford both profit and instruction.

VI.—Cholera, its Origin and Distribution.[1]

THERE is too much reason to fear that in the course of the

[1] 1. *Asiatic Cholera in Bristol in* 1866. By WILLIAM BUDD, M.D., F.R.S. Bristol, 1871.

2. *Seventh Annual Report of the Sanitary Commissioner with the Government of India*, 1871. Calcutta.

3. *Second Annual Report of the Sanitary Commissioners for the Central Provinces.* Nagpur, 1870.

4. *Cholera in Southern India: a Record of the Progress of Cholera in* 1870. By Surgeon W. R. CORNISH, Sanitary Commissioner for Madras. Madras. 1871.

5. *Report on the General Aspects of Epidemic Cholera in* 1869: *a Sequel to a Report on the Cholera of* 1866-68. By JAMES L. BRYDEN, M.D., Statistical Officer attached to the Sanitary Commissioners with the Government of India. Calcutta, 1870.

6. *Note on the Epidemic Connection of the Cholera of Madras and Bombay with the Cholera Epidemics of the Bengal Presidency.* By JAMES L. BRYDEN, M.D. Calcutta, 1871.

7. *Report on Measures adopted for Sanitary Improvements in India, from June,* 1870, *to June,* 1871. Printed by Order of the Secretary of State for India in Council. London, 1871.

8. *Hindu View of Cholera.* By GOLAUB SING, M.D. London, 1871.

9. *Mode d'Invasion et de Propagation du Choléra étudié à Smyrna.* Par le Dr. CHASSEAUD. Constantinople, 1871.

10. *Epidemic Cholera,* in *Reynolds's System of Medicine.* Vol. I. By Dr. GOODEVE.

11. *Report on the Cause and Mode of Diffusion of Epidemic Cholera.* By WM. BALY, M.D. London, 1854.

12. *A Treatise on Asiatic Cholera.* By C. MACNAMARA. London and Calcutta, 1870.

13. *Etude Sommaire sur l'Importation du Choléra, et les moyens de le prévenir.* Par le Dr. SIRUS PIRONDI et le Dr. AUGUSTIN FABRE. Marseilles, 1865.

14. *Ninth Report of the Medical Officer of the Privy Council.* London, 1867.

15. *On the Mode of Communication of Cholera.* By JOHN SNOW, M.D. London, 1855.

present year cholera may reach our shores and prevail once more as an epidemic pestilence.

The possibility of such a visitation renders it desirable to review the evidence which we possess as to the manner in which this formidable malady originates and spreads.

The men who have contributed most to our knowledge of the origin and propagation of cholera have been those who, laying a firm hold upon certain unquestionable facts, have perseveringly applied them in explanation of the phenomena of the disease. Amongst these Dr. William Budd and the late Dr. Snow deserve honorable mention. Dr. Snow was the first to direct attention to the influence of impure water as a means by which the disease is communicated, and by his persevering efforts, in the face of ridicule, opposition, and sceptical criticism, he compelled attention to this most important subject. His error consisted in the adoption of a too exclusive theory. He maintained that the essential cause of cholera is the swallowing of the specific discharges which have been expelled by a previous sufferer from the disease. He believed that the poisonous secretions, being swallowed either with food or drink, but most commonly with contaminated water, are thus brought into contact with the mucous membrane of the alimentary canal, where they undergo a rapid increase, and give rise to the phenomena of the disease (15, p. 1).[1] Now, it is certain that cholera not unfrequently originates under circumstances which forbid us to assume that either the food or the drink has been the vehicle of the poison. We believe with Dr. William Budd that the poison of cholera is cast off with the characteristic discharges from the alimentary canal, and that the disease is communicated to others in the following principal ways:—1. By the soiled hands of the attendants, who are not careful to wash before taking their food. 2. By means of bed and body linen, carpets, and other articles, soiled by the choleraic discharges. 3. The discharges, finding their way into the sewers and into the soil, may spread the disease in various ways—(*a*) by percolating through the soil into wells and other reservoirs of drinking water; (*b*) by rising with watery vapour into the air; (*c*) by becoming dried, and then dispersed with the atmosphere in form of an impalpable dust. In the words of Mr. Simon, "it cannot be too distinctly understood that the person who contracts cholera in this country is, *ipso facto*, demonstrated with almost absolute certainty to have been exposed to excre-

[1] We have numbered the various publications which we shall have to quote in the course of this article, and to prevent the necessity of repeating the titles of the works we shall refer to them by giving the numbers corresponding with those at p. 56.

mental pollution—excrement-sodden earth, excrement-reeking air, excrement-tainted water; these are for us the causes of cholera." In the fact, then, that cholera is communicable from the sick to the healthy through the medium of the specific discharges, we have conclusive evidence that the disease is eminently contagious. By contagious we mean communicable from the sick to the healthy, not by contact with the bodies of the sick, but with a material poison thrown off from their bodies and capable of being conveyed to a distance.

The non-recognition of the contagiousness of cholera, as the central fact in the history of the malady, has been the source of infinite confusion amongst medical writers, and of numberless preventible disasters in many parts of the world.

We propose now to refer to the evidence in favour of the proposition that cholera may be conveyed from an infected district to one previously healthy by human agency. The evidence upon this point is so abundant that the difficulty consists in selecting from a great mass of material. In Dr. Baly's Report (11, pp. 296 to 318) there will be found the particulars of the appearance of cholera in seventy-three towns, villages, and public establishments in England during the epidemic of 1848-9, subsequent to the probable introduction of infection. These numerous cases afford most indisputable evidence that cholera is a portable and a contagious disease. As an example, we select one case illustrative of the introduction of cholera into Titchmarsh, near Thrapstone (11, p. 300):

1. A butcher had been to Peterborough, and visited a house there, infected with cholera, on the 3rd October. His bowels became very loose on the morning of the 6th; he soon became collapsed, and died the same night.

2. His mother was attacked on the 13th, and lingered to the 24th. His wife, two daughters, and a son, were also ill with severe diarrhœa.

3. The woman who nursed the first case was attacked in his house on the 13th; moved to her own, 400 or 500 yards off, on the 14th; recovered.

4. This woman's daughter was attacked on the 17th; removed to her house, half a mile off; recovered.

5. The husband of the last patient was attacked on the 22nd.

Nine cases (6 fatal) occurred within a few doors of the last patient between the 21st October and the 15th November. The sister of Case 5, who nursed Cases 4 and 5, was attacked on the 4th November, and died in twelve hours. Two men living next door to the first case were attacked on the 9th and 14th November; both died. A woman who nursed them was attacked on the 17th, and died in twelve hours.

There is abundant evidence to show that in numerous instances cholera has been imported by a ship having the disease on board. A remarkable instance is recorded by Deputy Inspector-General Barrow ('Statistical Sanitary and Medical Reports, Army Medical Department,' London, 1866):—On the 9th April the steam-ship England arrived off Halifax (Nova Scotia), having cholera on board. A pilot, with an assistant and his son, went off to the ship in an open boat. Hearing that there was sickness on board, the men remained in the boat, which was towed at a considerable distance astern by a ten-fathom rope. In this way the ship, with the boat in tow, was conducted to her place of anchor. The pilots then went on shore, and were never on board the infected ship.

In the course of the next two days both men were seized with cholera, and one died. Both men communicated the disease to their families. In the family of one there were three cases, but no death; in that of the other, besides the father, who died, there were four cases and two deaths. The only other cases of cholera at Halifax at that time occurred in the family of a man whose two children were seized after playing with some soiled bedding which had floated on shore from the infected steamer; one child died, the other recovered. The mother also took the disease, and died. Surely if disease was ever communicated by infection it was so in the case of these three families, the different members of which were in various ways brought into contact with the infecting material imported by the steamer.

We would remark in passing that Dr. Chasseaud's little work (9) contains most conclusive evidence of the importation of cholera, and its subsequent diffusion by contagion in Symrna and in the surrounding villages during the epidemics of 1848 and 1865, while in the work of Drs. Pirondi and Fabre (13) there is equally satisfactory evidence of the importation of the disease into Marseilles in 1865, and of its conveyance by human agency from one part of France to another during the earlier epidemics (see also that part of Dr. Baly's report which relates to the introduction of cholera into islands or new continents by human intercourse (11, pp. 139 to 155). An indirect proof of the contagiousness of cholera is afforded by the fact that the disease has never reached certain countries which, being separated from India by a wide expanse of ocean, and having little communication with that country, have escaped the infection. Amongst these countries are Australia, New Zealand, and the other Pacific Islands; the Cape of Good Hope and the southern part of the west coast of Africa; the Azores, Bermuda,

Iceland; the Faroe, Orkney, and Shetland Islands, and the western coast of South America.

In India it is, perhaps, more difficult to get conclusive evidence of contagion than in Europe, yet the records of the disease in India afford convincing proof of contagion. For example, Dr. Goodeve (10, p. 680) quotes from the report of the Indian Cholera Committee on the epidemic of 1861 some remarkable facts with reference to the introduction of cholera into the villages of British Gurwhal within the Himalaya mountains. In the early part of 1852 extensive works of irrigation at the foot of the mountains brought together several thousand workmen. Cholera broke out amongst them, and they fled, panic stricken, to their homes, which were generally at a distance of several days' journey in the interior of the hills. Up to this time cholera had been unheard of amongst the mountains. Many of the workmen died on their way home, many others were attacked after they reached their villages. Then cholera broke out among the other inhabitants of the villages, commencing in very many instances in the families of the men who had brought the disease from below. For a considerable time cholera was entirely confined to the places which had been in direct communication with persons suffering from the disease, but in the course of a few weeks it had become impossible any longer to trace such connection, and cholera became generally epidemic in the hills. Many of the first cases were carefully investigated, and it appeared to be proved beyond the possibility of a doubt that cholera was brought by human intercourse into a district which, up to that time, had been free from every sign of the disease.

Dr. Townsend, the Sanitary Commissioner for the Central Provinces of India, in his second report (3, pp. 168-9, &c.) shows that every district in which inquiries were instituted furnishes evidence of importation of the disease by human intercourse, and of its subsequent spread from person to person and from village to village.

It appears scarcely necessary to multiply evidence of this kind to prove that cholera may be conveyed from place to place by human agency.

In proceeding to discuss the precise mode in which the disease is communicated we shall have no difficulty in proving that the discharges from the stomach and bowels have the power to impart the disease. The following statement by Dr. C. Macnamara (12, p. 196) affords conclusive evidence upon this point. He says—

"I may mention the circumstances of a case in which the most positive evidence exists as to the fact of fresh cholera dejecta having

found their way into a vessel of drinking water, the mixture being exposed to the heat of the sun during the day. Early the following morning a small quantity of this water was swallowed by nineteen persons.

"They all remained perfectly well during the day, ate, drank, went to bed, and slept as usual. One of them, on waking the next morning, was seized with cholera; the remainder of the party passed through the second day perfectly well, but two more of them were attacked with cholera the next morning; all the others continued in good health till sunrise of the third day, when two more cases of cholera occurred. This was the last of the disease; the other fourteen men escaped absolutely free from diarrhœa, cholera, or the slightest malaise."

The author states that at the time of this remarkable occurrence, which could hardly have been the result of accident, there had been no cholera in the neighbourhood for several years, nor, so far as he is aware, has it appeared there since. The only sufferers from the disease were five out of nineteen men who had on one occasion drunk water contaminated by choleraic discharges. We learn from this narrative that cholera may be communicated by the mixture of choleraic discharges with drinking water; that after the poison is swallowed there intervenes a period of incubation, during which the poisoned man remains apparently well. We also learn that out of a certain number of men who have imbibed the cholera poison the majority may escape unharmed. In other words, some men are more susceptible than others. The fact, therefore, referred to by Dr. Planck, the Sanitary Commissioner for the North-Western Provinces (2, p. 118), that "wherever cholera appeared those attacked and those who escaped drank from the same wells," need not shake our belief in the power of contaminated water to convey the disease. It is notorious that the most unquestionably contagious diseases are not communicable at all times to all persons who are exposed to the influence of the poison. There are on record numberless instances of persons who have resisted the poison of smallpox, although again and again exposed to the contagion, yet at a subsequent period they have succumbed to the disease.

The influence of impure water in favouring the spread of cholera and in increasing the mortality from the disease is proved by the most conclusive evidence.

In illustration of this we may refer to the notorious outbreak of cholera in the eastern districts of London during the last epidemic of 1866.

The average rate of mortality from cholera per 10,000 population throughout the whole of London was 18·4. Out of the thirty-six registration districts into which London is divided, in

only seven did the mortality exceed the average. Of these, in Greenwich town district the mortality was 20·7, while in the six eastern districts the lowest rate was 60·4, and the highest 107·6. These figures show the enormous preponderance of the disease in the eastern districts of London (14, p. 280, &c.).

Mr. Radcliffe's very elaborate and careful inquiry into the circumstances of this terrible outbreak led him to the conclusion that the water from certain reservoirs of the East London Water Company "had in it a power of direct infectiveness for those who drank it;" and Mr. Simon, while admitting that some apparent contradictions are difficult of explanation, says, "After giving my most careful judgment to the case, I feel bound to say that the facts adduced by Mr. Radcliffe seem to me, as a whole, almost irresistibly to force the substance of his conclusion on the mind" (14, p. 366).

Again, it was shown by Dr. Snow, by the Registrar-General, and by Mr. Simon, that during the epidemic of 1854 the mortality in the south districts of London was greatly influenced by the impure water supplied by one of the two water companies. The Lambeth Company drew its water from the Thames at Ditton, above the influence of the tide and of the London sewage, while the Southwark and Vauxhall Company at that time supplied impure water from the river near Vauxhall. The two companies distribute their water in the same district and to the same class of people, the pipes of the two companies often lying side by side, and supplying neighbouring houses in the same street. The result was that amongst a population of from 400,000 to 500,000, the mortality was three and a half times as great amongst those who drank the foul water as amongst those who had a comparatively pure supply. The celebrated and fearful outbreak in the neighbourhood of Golden Square in 1854, which was traced to the contaminated water of the Broad Street pump, affords a striking illustration of the influence of impure water. Upwards of 500 deaths occurred in a very limited district, and the greater number within a period of ten days at the end of August and the beginning of September, 1854. The two most remarkable cases which were traced to the water of the Broad Street pump were those of a woman living at Hampstead, and a niece who was on a visit to her. They both drank the water from the Broad Street pump, which was carried daily to Hampstead, a distance of nearly four miles. The aunt died of cholera, after a few hours' illness, at Hampstead, and the niece after she had returned to her home, in a high and healthy part of Islington. There were no other cases of cholera at that time either at

Hampstead or in the neighbourhood where the niece died (15, p. 44).

It is interesting to note the fact that since Calcutta has been supplied with pure water there has been a great diminution in the number of deaths from cholera. During the year 1870, the first year of the new water supply, the deaths from cholera were only 1563, this being little more than half the lowest mortality during any previous year in which statistics are available (7, pp. 16, 17).

We shall presently return to the influence of impure water; meanwhile we will refer for a moment to the communication of cholera by means of contaminated clothing.

Dr. Snow quotes the following cases as having occurred in 1832 (15, p. 4):

At Moor Monkton, a healthy agricultural village in Yorkshire, John Barnes, an agricultural labourer, was seized with cholera at a time when the disease was not known to exist within thirty miles of the place.

On the following day the patient was dead, and his wife and two men who had visited Barnes on the previous day were labouring under the disease, but recovered. Three other persons who had communicated with the patients had diarrhœa. While the surgeons were vainly endeavouring to trace the disease to its source, the mystery was cleared up by the arrival of the son of the deceased John Barnes. This young man was living with his uncle at Leeds. He informed the surgeons that his aunt had died of cholera a fortnight before, and that, as she had no children, her wearing apparel had been sent by a carrier to Moor Monkton. The clothes had not been washed, and Barnes had opened the box on the evening of the day before he was seized with cholera.

Dr. Baly's Report shows (11, pp. 172-3) that in a large number of cases the disease was believed to have been communicated by soiled clothing.

It is stated that in the Wandsworth Union Workhouse five laundresses were attacked, and two died; and as there were in all only eight deaths from cholera in the workhouse, it is probable that the women engaged in washing the linen suffered in a much larger proportion than the other inmates of the house. The following striking example of infection by clothing is given by Dr. Baly (11, p. 297):—A gentleman who had come from Bath to attend the funeral of a friend who had died of cholera in London, took the disease and died at Sydenham. The second, third, and fourth cases that occurred at Sydenham were sons of the woman who washed the linen and clothes of the first patient. They were washed in a yard to which the second

and third patients (children of eight and ten years) had access, and the fourth patient, a young man of twenty-five years, had carried the unwashed clothes from one house to the other. These cases, which were all fatal, occurred a few days after the first case. Only one other fatal case occurred at Sydenham.

The conclusive evidence afforded by such cases as these is in no degree impaired by any number of instances in which the washing of infected clothing has not been attended with the like disastrous results. The danger incurred by those who are engaged in washing contaminated clothes will depend upon a number of circumstances—upon the varying degrees of susceptibility of different individuals, upon their habits of personal cleanliness or the reverse, upon the size and ventilation of the workrooms, upon the immersion of the clothes in a disinfecting liquid prior to the operation of washing or upon the neglect of this precaution, and, lastly, upon the physical condition of the dejecta with which the clothes have been soiled. There is good reason to believe that the fresh cholera stools are nearly, if not quite, harmless; that their greatest infecting power coincides with the stage of commencing decomposition, and that in a more advanced stage of decomposition they again become harmless.

With reference to this point, Mr. Macnamara says (12, p. 396):

"I am prepared to state that the contaminated water, which was certainly poisonous during the vibrio stage of decomposition of the cholera stuff, may be drunk with absolute impunity after the bubbles of air have begun to form in it, and the confervoid growth has taken the place of most of the ciliated infusoria. Beyond this I have very strong grounds for believing that immediately the organic matter has passed through its vibrio stage of decomposition it becomes harmless; that it would not produce cholera, if swallowed by another person, when the vibriones had disappeared, and ciliated infusoria had taken their place."

This opinion, he says, "is based upon tolerably certain grounds;" and, in fact, his statements suggest the inference that his conclusions are the result of experiments, the details of which it would, perhaps, not be expedient to publish. Mr. Macnamara also states that if fresh cholera dejecta are dried on clothes or furniture, or in the soil, they retain their activity for years if access of moisture be prevented. He had some of the dejecta which, when fresh, were mixed with sand and dried seven years before. Now, when mixed with water and exposed to the sun, they could not be distinguished from a fresh cholera stool. This explains the danger of visiting the site of a former encampment, the soil of which has been contaminated by choleraic discharges.

We have referred to facts which prove that cholera is communicable by the intestinal discharges, and that either contaminated water or soiled linen may be the vehicle of the specific poison. It remains to be shown that the virus may be diffused through the air, and thus enter the system by the lungs. In proof of this proposition we will refer to some facts, recorded with singular clearness and ability by Dr. Wm. Budd, in a series of letters under the signature of "Common Sense," in the 'Association Medical Journal,' 184-54.

In the autumn of 1849, a workhouse, in an open country on high ground, contained 750 paupers. The health of the inmates was good until a woman brought cholera from a city three miles distant, where the disease was rife. Immediately on her arrival, on the 16th August, she was under treatment for diarrhœa, and on the 18th she died of cholera. On the 21st August there was another case, then another, and another, until, in little more than a week, there were more than fifty cases of cholera at once. Within five weeks after the occurrence of the first case nearly 100 inmates had taken flight, and out of the remaining 645, 144 had died of cholera. A careful inquiry into all the circumstances of this fearful outbreak led to the conclusion that the disease was propagated mainly through the agency of ill-constructed privies, which became contaminated by the discharges from the sick while they were daily resorted to by the healthy. One of the most conclusive pieces of evidence in support of the proposition that the privies were the source of infection is contained in the following narrative:—On one floor of a building there were two wards, separated by a narrow passage; each ward was occupied by thirteen men. In ward A there were seven cases of cholera and six deaths. In ward B there was only one case. It should be noted that each case as soon as it occurred was removed to a building set apart for a cholera hospital. Now, what is the explanation of the almost complete immunity of ward B as compared with the high mortality in ward A? The men had the same food and water and breathed the same air, but the inmates of ward A were healthy men, who frequented the tainted privy, while ward B was a sick ward, and all its occupants but one were confined to bed. The only inmate of this ward who was able to leave his bed, and who, therefore, habitually went to the privy, was the only one who got cholera. As soon as the symptoms commenced he was removed to the cholera hospital, where he died.

Lest it should be supposed that the other twelve inmates of ward B owed their safety to the mere fact of their being invalids, and not to their exclusion from the infected privy, it may be well to note the fact that on the women's side

of the building there was a sick ward, with six women in it. Early in the outbreak this ward was made a cholera ward. Cholera was thus brought to these six women, and the result was that *they all took it and all died of it.* There was no other instance of such wholesale mortality within the building.

In those portions of the building which were occupied by women there were, at the commencement of the outbreak of cholera, 244 women and infants. For the use of all these persons there was one common privy. Of these inmates, a considerable number fled. Of those who remained, 81, more than 1 in 3, had cholera, and 74 died. Among the wards was one for 19 women under treatment for surgical complaints. Of these 19 women, 6 had cholera, and 4 died. Of the 13 who escaped, there were 4 who were confined to their beds, they being the only 4 who were so, while of the 15 who were able to leave their beds, and who, therefore, resorted to the common privy, 6 had cholera. There were other striking instances of wholesale exemption within the same establishment. We have seen that out of the total number of women 81 had cholera, and 74 died. In every ward a large proportion had the disease; in many wards one half took it; in one ward all died. Now, within a few paces of the spot where this appalling mortality was occurring there were 16 persons housed in a separate building, who all escaped. Of these 16 persons, *who were fed from the same table and drank of the same water* as their neighbours who were dying by the score, not one had even diarrhœa. Fortunately for these 16 persons, they were all suffering from contagious forms of skin disease, either itch or scald head. They were, in consequence, confined closely to their rooms, and, in particular, *they were strictly debarred from resorting to the common privy.* It can scarcely be doubted that this enforced seclusion, which was designed for the protection of others, afforded to this fortunate 16 a harbour of refuge from the storm that was raging around them. But still more striking was the almost complete exemption of 66 children who lived in a separate building in another part of the place. In this building there was a privy common to the children, and devoted to their sole use. This privy drained into a cesspool, which had no connection with the common drainage of the place. A single case of cholera occurred among these children early in the outbreak, but the subject of it being removed elsewhere immediately after the first seizure, the child recovered, and no other case occurred amongst the children. Yet the building in which they lived stood close by another where, out of 120 inmates, there were 28 who died, and only 6 who were not attacked. A consideration of all the circumstances of this fearful outbreak of cholera leads irresistibly to the conclusion

that the disease, having been introduced from without, was propagated by the discharges from the sick, and that the rapidity and extent of its spread were due to the fact that so large a proportion of the inmates of this plague-stricken establishment were brought into close relation with the discharges through the poisoned air of the privies, which had become the receptacles of them. We cannot refrain from adding that, if all the circumstances of every outbreak of cholera were investigated with the same untiring energy and sagacity as were displayed by Dr. Wm. Budd in this most interesting inquiry, we should soon cease to hear of outbreaks of the disease being so mysterious and inexplicable as to justify the Dundreary lament that "no fellow can understand them."

Dr. Budd refers to an instance in which the air of a coal mine became infected. A man, suffering from the disease descended into the pit, and while there he had copious discharges. He was brought up in a state bordering on collapse, and in a few hours he was dead. On the following day the man who had worked by his side, and had helped him home, was attacked, and died. Then came another and another case, and before many days had passed 17 out of the 40 men employed in the pit had been attacked, and 9 were dead. Diarrhœa was universal in the pit, and it became so evident to the men that the source of infection was in the pit that to a man they refused to go down. It is not uninteresting to remark that the more intelligent among them ascribed their illness to the effluvia from the discharges left in the pit by the first sufferers from the disease.

In further illustration of this subject we propose to refer to Dr. Parkes's interesting account of the introduction of cholera into Southampton in the year 1866 (see 14, p. 244):—On the 10th June the Peninsular and Oriental Steamship Poonah arrived from Alexandria, Malta, and Gibraltar. On the voyage home the crew and passengers enjoyed perfect health until two days before arriving at Southampton, when several men became ill with severe diarrhœa, and one died of cholera. This outbreak was attributed to foul water, which had been taken in at Gibraltar. The tank containing this water was opened on the 5th June; the water had a foul smell and taste. It is believed that the firemen alone drank of this water; it is certain that on account of their occupation they drank more of it than any of the crew, and, with one exception, the firemen alone suffered from choleraic disease.

On the 8th June one fireman went on deck and drank a large quantity of this water. Early the next morning he was seized with vomiting, purging, and cramps; he became cold and

pulseless, and died in nine hours. On the day of his death, June 9th, six or seven other firemen, and on the following three or four more, were affected with violent purging, and some with vomiting as well. None of these men reported themselves to the medical officer, as they were afraid of being detained on board, and they all landed at Southampton and dispersed themselves over the town on the 10th and 11th of June. They were seen by several medical men, who diagnosed the disease as the severest "choleraic diarrhœa." One case, to be presently referred to, terminated fatally. The men all attributed their illness to the foul water. All who were ill had drunk of the water, but it is not certain that all who drank of it became ill. The passengers did not drink of it, and none were attacked; only one man of the crew had diarrhœa. The water was found on analysis to contain much organic matter, the result of contamination by sewage, but there was no cholera at Gibraltar when the water was taken on board. For a more detailed account of this water we must refer to Dr. Parkes's narrative. Edward Palmer, one of the firemen in the same watch as the man who had died on board, went to his home, in a very clean and airy situation, on Sunday, June 10th. Either on that day or on Monday he was attacked with diarrhœa, which was very bad on Monday, Tuesday, and Wednesday. He slept in an airy room with his wife and child, generally using the closet outside, but once or twice the utensil in the room, which, however, he emptied immediately. On Wednesday, June 13th, his child, a boy, aged about three years, and previously in perfect health, was suddenly taken ill at 10 a.m. with violent vomiting, purging, and cramps; he soon became very cold, and died at 4 p.m. The child was seen by several medical men, who were satisfied that the case was one of cholera. On the Thursday Palmer himself became worse; the eyes were sunken, the hands shrivelled; he passed no urine; and he died on Friday, June 15th. No other case occurred in the house or neighbourhood, and Dr. Parkes ascertained by careful inspection that there was no local cause to account for the child's illness, which must have been communicated by the father while the disease was yet in the diarrhœal stage, and before the distinctive symptoms of cholera had appeared. It can scarcely be doubted that all the men who landed from the Poonah with so-called diarrhœa suffered from the same disease as the man who died after a few hours' illness on board, and Palmer and his child, who died on shore, and that this disease was cholera. This being admitted, there can be no doubt that the dejecta of cholera in large quantities must have passed from eight or ten persons into the sewers of the town,

and in this way there is reason to believe that cholera was introduced into Southampton.

Besides the Poonah cases, 4 doubtful cases occurred in Southampton, 1 on the 12th and 15th June, and 2 on the 6th July, but in the early part of July, three weeks or a month after the arrival of the Poonah, the outbreak decidedly commenced. On the 5th there was 1 case; on the 11th, 1; on the 12th, 1; on the 13th, 9 cases; about the same number on the 14th, and 10 on the 15th and 16th. Up to the evening of July 17th probably no less than 35 to 37 deaths had occurred. On the following day the numbers were not quite so great, and on the 24th July the numbers were greatly decreasing. By the 4th or 5th of August the outbreak was virtually over, though a few cases occurred through the rest of August. Even in September 7 deaths were registered from cholera, and 2 in October. What, then, was the cause of this outburst? The disease was confined to the low and unhealthy parts of the town, but it was scattered through them, and not extremely bad in any one spot. All the upper part of the town was free, and the surrounding suburbs and villages remained unaffected. The following conclusions were arrived at by Dr. Parkes, after a careful collection and consideration of evidence. Except the Poonah, there is no traceable mode of importation, though, as the disease existed in many parts of the Continent and of England, other channels are possible.

If introduced by the Poonah, it did not, except in the case of the fireman's child, spread to others by close proximity. Although on board the Poonah the disease was caused by bad water, its subsequent outburst in Southampton was not due to impure water. The whole of Southampton has a continuous supply of pure water. The poison was probably disseminated through the sewers. At the lower part of the town the sewers are ill ventilated; the sewage accumulates, and gases are often forced into the houses. Just before the outbreak the pumping of sewage had been discontinued while the sewers were being cleansed. The pumping was resumed at the commencement of July, several tons of offensive sewage being daily poured down an open conduit into the outlet sewer. The odour was overpowering, and was bitterly complained of in the adjacent houses. Several cases of diarrhœa and cholera occurred in clean, airy houses near the pumping station, for which no other cause could be discovered than the effluvia from the sewage.

As soon as this was discovered means were taken to substitute a closed iron pipe for the open conduit, and carbolic acid was largely introduced at this point into the sewer. The alteration was completed late at night on the 18th July. Up to that date

the cases had been very numerous; they then lessened, and on the 24th July it was evident that the worst was over. Dr. Parkes expresses his belief that the outbreak was mainly caused by the discharge from the pumping station into the air of fæcal effluvia from sewers into which cholera dejecta had been largely introduced, and that the rapid decline of the disease was owing to the removal of this cause of foulness of the air. It is also suggested as probable that some cases were caused by the escape of poisonous gases into the houses through inefficient traps, and that in this way may be explained the few scattered cases that occurred in August and September.

We have given examples, selected from a large number lying ready to our hand, illustrative of the various modes in which cholera arises and spreads. There yet remain some questions to be considered.

It is now generally admitted that impure water is a most influential agent in the causation of cholera. It has been proved by Mr. Macnamara, as Sir Thomas Watson remarks, "with all the force, if not with the reality of an experiment," that drinking water contaminated with the choleraic discharges is alone sufficient to excite an attack of cholera. Now, in connection with this subject two questions arise—(1) Will the drinking of water contaminated with organic impurities, but not containing choleraic secretions, cause the specific disease cholera; in other words, may the disease arise *de novo* from drinking filthy water? (2) Is the spread of the disease under the influence of impure water a proof that the water is contaminated with choleraic discharges! We believe that both these questions should be answered in the negative. Dr. Townsend has shown in his report (3, p. 177, &c.) that the inhabitants of a town or village may use water impregnated with organic impurities to an extreme degree with apparent impunity, until a sufferer from cholera arrives among them, when the disease spreads with violence and rapidity proportioned to the degree of pollution of the water supply. In illustration of this position he cites the case of Raipur, a town containing nearly 17,000 inhabitants (p. 179). A large proportion of the population had for many weeks been drinking most impure water. On the 15th and 16th of May, many persons suffering from cholera reached the town. The disease at once began to spread, and in ten days it had reached its culminating point. It then began to decline, and in little more than a month it had ceased to be epidemic. In the course of the month 1154 persons were attacked, this number representing the proportion of persons who were rendered susceptible of the poison by the conditions under which they were living. Those who are imbibing impure water are especially predis-

posed to suffer from the specific action of an air-borne poison. In like manner, putrid food, whether animal or vegetable, may supply the requisite predisposing condition; so also may the accumulation within the system of organic impurities, the result of over-crowding, of defective drainage, or of disease affecting one or more of the great blood-purifying organs. Mr. Cornish, in his very able and interesting report (4, p. 116), describing the history of an outbreak in the 45th Regiment, states that the first case occurred in a sergeant, "a man in delicate health, and suffering habitually from offensive breath, so that his comrades spoke of him as being 'rotten' in constitution."

We have now no difficulty in understanding that cholera is a disease capable of being rapidly communicated from the sick to the healthy over an extensive town population, and yet that, with ordinary precautions, the immediate personal attendants on the sick incur scarcely any risk of infection. In this respect there is a very close resemblance between cholera and enteric fever. The alimentary canal is, in both diseases, the chief, if not the sole, outlet for the poison. The bowel discharges are the means of infection, and whereas the fresh secretions are comparatively innocuous, the subsequent fermentation of the discharges from a single patient may infect the drains of an entire district. Hence it follows that the inhabitants of a house with untrapped drains, half a mile or more away, may incur three times more risk of infection than the sick man's nurse or doctor. It is probable that in the fact that, until quite recently, the Thames has been the common sewer of London, receiving, therefore, the poisonous discharges from the entire city, we have the explanation of Dr. Wm. Farr's "law of altitude." The lower the situation—in other words, the nearer and the more exposed to the poisonous emanations from the river—the greater the fatality from the disease. That mere altitude affords no protection is shown by the occasional prevalence of cholera in the elevated mountain districts of India. (See 10, p. 673, and 3, p. 178.)

The history of epidemic cholera shows clearly that the poison of that disease is diffusible through the air to a greater distance than that of most, if not of all other infectious diseases. It can scarcely be other than an air-borne particle of poison that occasionally, during an epidemic of cholera, picks out a solitary prisoner in the midst of a healthy establishment. And the rapid diffusion of the poison through the air affords the only intelligible explanation of the quick extension of the disease from a few infected centres over a densely populated town. Of course, the distance to which the poison may be carried is limited. Dr. Townsend says "there is no evidence

that it can be conveyed a mile by the wind" (3, p. 165). The poisons of smallpox, typhus, &c., are, happily, portable by air to a far less extent.

As in civilised town populations the drains are the chief means by which the cholera poison is diffused into the air, and so scattered through a district, it is obvious that one of the chief means of preventing the spread of the disease consists in the thorough disinfection of all choleraic discharges before they are allowed to enter the drains. By this means a threatened conflagration may be stamped out and extinguished. What a saving of life might have been effected if the discharges from the first patient in the workhouse referred to by Dr. Budd had been disinfected and kept out of the common privy!

Dr. Budd points with pride and pleasure to the happy results of the energetic measures adopted for stamping out cholera in Bristol in 1866; but in instituting a comparison between the small mortality in Bristol and the terrible fatality of the disease in the eastern districts of London, he appears to allow too little influence to the pure water at Bristol and the filthy liquid supplied for a time to the inhabitants of East London. If the water contamination had occurred at Bristol instead of at East London, other conditions remaining the same, the result would have been very different.

The question has often been raised—Why is cholera only an occasional visitant beyond the limits of its endemic home in certain provinces of Bengal? To this question we can at present give only an incomplete answer. It seems probable from analogy, that some unknown atmospheric conditions favour the extension of the disease in an epidemic form at certain seasons, and not at others. We know that, as a rule, the spread of cholera is favoured by a high temperature, and that its progress is checked by cold. There have been serious local outbreaks during the winter season, both in India and in Europe, but cholera has rarely, if ever, existed as a widely diffused epidemic except during the warm months of summer and autumn. Neither great moisture nor extreme dryness of air is essential for the prevalence of the disease, which has been widely diffused and very virulent under the opposite extremes of moisture and of dryness. The disease has sometimes been known to break out with sudden violence after the occurrence of a mist-laden or a dust-laden wind blowing from a neighbouring infected district, and it has sometimes received a sudden check after the occurrence of a violent storm. If the conditions which render cholera epidemic at one time rather than another are in great part unknown, we are equally ignorant of the circumstances which, at uncertain intervals, promote the epidemic pre-

valence of such infectious diseases as smallpox, typhus, and relapsing fever.

The rapid decline of an epidemic of cholera after it has reached its culminating point is probably explained by the fact that when the poisonous influence has become widely diffused, all who are susceptible of the disease are quickly affected. The majority of the population are happily not susceptible, and the disease ceases for want of individuals on whom the poison can act, and in so doing propagate itself. In an analogous manner, when measles enters a large public school, it quickly affects all who are unprotected by a previous attack, and then the epidemic ceases.

Dr. Bryden, the able statistical officer attached to the Government Sanitary Commissioner, maintains that the spread of cholera is effected almost entirely by atmospheric waves, and that human agency has but little influence upon the progress of an epidemic (see 5 and 6). This theory, which appears to us to be quite inconsistent with the unquestionable facts of the disease, is viewed with favour by the Sanitary Commissioner with the Government of India (see 2, p. 122, &c.), but it is very ably criticised by some of the provincial sanitary commissioners, especially by Mr. Cornish (4, p. 141, &c.), by Dr. Townsend (3, p. 161), and Dr. De Renzy (2, p. 118). They show that atmospheric currents are alone quite insufficient to explain the spread of cholera, which, starting from a centre, and taking the lines of human traffic, radiates towards all points of the compass, and often makes progress in spite of opposing winds.

It appears to us that Dr. Bryden's theory naturally tends to discourage sanitary measures, and to favour a fatalistic view of the nature and cause of cholera, not unlike that which is set forth by the Hindoo physician, Dr. Golaub Sing (see 8). Dr. Sing ridicules our search for the causes of cholera. He has been taught that "the eternal infinite Brahma, ever self-expanding and suffusing, becomes and is all existence." The result is, that "cholera and fevers are evolved with and of the nature of man," who apparently can do little more than look on and witness this fatal "evolution." We think, however, that the philosophic Hindoo might learn some useful lessons from the practical Europeans, whom he looks down upon as "a recent civilisation, hardly yet emerged from the period of assertion and definition!"

We had intended to discuss the pathology and treatment of cholera, but we have already exceeded our allotted space; we must, therefore, defer the further consideration of this subject to a future occasion. Meanwhile, we desire to direct especial attention to Sir Thomas Watson's rewritten account of cholera

in the new edition of his lectures. The whole subject, including the mode of diffusion of cholera, the pathology, and the treatment, is there discussed with such rare ability and judgment as cannot fail to produce a deep impression upon every unprejudiced reader.

In the unhappy event of cholera again becoming epidemic, Sir Thomas Watson would confer a great benefit upon the public by allowing a reprint of that lecture, in the form of a pamphlet, for general distribution. It would be extensively read, highly appreciated, and extremely useful.

VII.—English and Scottish Lunacy.

The reports for the past two years of the Commissioners in Lunacy for England and for Scotland, both from the importance of their subject-matter, and of the facts to be found in their pages, call for examination in this review. The subject of lunacy certainly yearly challenges increased consideration, and indeed assumes the dignity of a state question of no mean proportions.

Whatever be the explanation, the fact remains, that every year adds considerably to the pre-existing amount of insanity present in this kingdom. Not only does the grand total of cases increase, but the ratio of the insane to the entire population also progresses in a very distinct and seemingly nearly regular ratio; and it further comes to pass that the vast efforts made to overtake the demands for the accommodation and treatment of lunatics fail to meet them, and the once prevalent philanthropic ambition of securing asylum accommodation for all the victims of mental disorder in the kingdom is now practically relegated to the category of Utopian projects.

We shall not enter on a disquisition respecting this growth of insanity, nor discuss whether it is a positive or only an apparent increase, or whether the present generation is more mad than the preceding one; but will take the facts and figures contained in the twenty-fourth and twenty-fifth reports of the English, and in the thirteenth and fourteenth reports of the Scottish Commissioners in Lunacy, and make some comments thereupon so as to assist our readers in forming an opinion regarding the present state of lunacy in England and Scotland.

Those reports present respectively the state of lunacy in England and in Scotland during the two years ending January 1st, 1870, and January 1st, 1871.

On the first of January, 1870, the whole number of persons of unsound mind in England and Wales, known to the Commis-

sioners, reached a total of 54,713 and showed an increase during the year 1869 of 1536; on the first of January 1871, the total amounted to 56,755, and the year's increase to 2042. At the former date the number for Scotland stood at 7571, being an advance of 261 on the year preceding; at the latter the total had advanced to 7729, yet the increase in the year was only 158 as against 261 in the twelvemonth preceding, and so far, therefore, represented a declining ratio of lunacy in the population.

The addition of 1536 lunatics to the population of England in one year, and of 2042 in that next following, looks certainly a formidable one; but on comparing the total number existing in each of those years with the population as annually augmented, the actual ratio of insane persons presents no such large dimensions. At p. 5 (Rep. 25th) the commissioners tabulate this ratio, and show that in the former year in question it was 2·47, and in the latter 2·49 per 1000 of the population, and that the average annual advance of this ratio during the last ten years has been a little short of ·50 per 1000; or, in other words there has been a positive increase annually of one lunatic in rather more than every 2000 individuals of the entire population of the country. However much, therefore, the question of the increase of lunacy in the country be debated, the table quoted is sufficient to determine it in the affirmative; although, on the other hand, it does not countenance the views of alarmists who look only to the gross annual augmentation as presented in reports. It is, at the same time, consoling to observe that since 1869 the ratio has not ranged so high as in previous years, and we may cherish the hope that the accumulation of by-gone years in chronic lunacy, gathered during the progressive development of asylums, is in natural course of decline by simple lapse of time.

Nevertheless, each year must leave a very considerable residuum to be added to the sum of accumulations of past years. This residuum in each year is represented by the difference between the total admissions, and the sum of discharges, and deaths. The English Commissioners present two tables; one of discharges or recovery, the other, of discharges of patients not recovered including transfers. It is the former table that alone represents an actual abstraction from the total number of lunatics; for both un-recovered patients sent forth into society, and patients transferred from one asylum to another, or to private houses, nevertheless continue members of the general community of the insane. Further, the table of admissions comprises the transfers, which should balance those enumerated in that of the discharges of uncured and the re-admissions; consequently the annual total exhibited surpasses that of cases

admitted for the first time by the sum of the re-admissions and transfers. As, however, the proportion of such re-admissions and transfers is not shown, we shall, in calculating the residuum year by year, as we propose to do for the ten years ending January 1871, deal with the totals as given in the table in question. Moreover, as the relative proportion of non-recovered patients discharged and of transfers is not exhibited in the table showing their totals, we shall omit that table. For although the numbers so recovered yearly differ, and would somewhat affect the residuum, making it appear smaller, particularly for the years 1868, 1869, and 1870, yet as we said before they do in no serious degree affect the real total of lunacy; except, indeed, so far as they may include a certain number of unrecovered lunatics sent out of the kingdom and withdrawn from the jurisdiction of the commissioners.

Adding now the discharged recovered, and the deaths year by year from 1861 to 1870, inclusive, and subtracting the sum so derived from the admissions in those several years respectively, the difference appears which we will call the residuum, and which represents the annual increment of the total lunacy in the kingdom. The results will be best seen in the following table:—

1861	the residuum	= 3,490	of	9,329	admitted, or 37·4	per cent.
1862	„	= 3,099	„	9,078	„ 34·1	„
1863	„	= 3,014	„	8,914	„ 33·8	„
1864	„	= 3,043	„	9,473	„ 32·1	„
1865	„	= 3,973	„	10,424	„ 38·1	„
1866	„	= 3,275	„	10,051	„ 32·5	„
1867	„	= 3,673	„	10,631	„ 34·5	„
1868	„	= 4,139	„	11,213	„ 36·9	„
1869	„	= 3,568	„	11,194	„ 31·8	„
1870	„	= 3,847	„	11,620	„ 33·1	„

At the present time we may therefore assert generally that one third of the numbers annually added to the total population of insane remain as further accretions to previous accumulation. But it is satisfactory to observe that, on comparing the per centage for the second period of five years with the first, there is a less average ratio, amounting to one and a half per cent., in the residuum.

Another subject deserving notice is the distribution of the insane and the relative number of private and pauper patients. Of 50,637 pauper lunatics existing on the first of January 1871, 27,109 were found in county and borough asylums; 1905 in registered hospitals and licensed houses; 12,161 in workhouses; 7331 residing with relatives or others; whilst 2131 are entered as in asylums, &c., chargeable to county and borough rates. The number of private patients are rated at only 5921; but

such of them as are placed singly, resident in their own houses or with strangers, and who amounted to 514, would seem not to be comprised in this number. They formed, however, only 174 of the total increase of 2042.

Of the pauper lunatics 61·25 per cent. are maintained in asylums, 24·18 in workhouses, and 14·57 with relatives or others. And the table (ix) from which these particulars are derived unfolds the unsatisfactory truth, that notwithstanding all the efforts made to provide proper asylum accommodation during the past twenty years, the advance made towards that object has been very trivial. Thus, on referring back to the proportion presented ten years since, we find that then 58·67 per cent. were maintained in asylums, 24·09 in workhouses, and 17·24 with relatives and others. It is in the last named form of accommodation that the greatest change has taken, but even here only to the extent of 2·67 per cent. The ratio of those found in workhouses continues virtually the same as it was ten years ago; and during the last three years that of the inmates in asylums has slightly declined.

The inference may be drawn from these statistics that the plan hitherto attempted of collecting the insane of all sorts into specially constructed asylums has proved a failure. At the same time it is only fair to note that, since the special report of the commissioners on the insane in workhouses, there have been great improvements in the accommodation furnished in those establishments, by the construction of special wards, suitably fitted; and particularly, within the last two years, by the erection of the commodious and ornate district asylums at Leavesden and Caterham for the occupation of the chronic insane of the metropolis; and, as we are reminded by the report, the inmates of these special structures are enumerated among the occupants of workhouses.

For many years past the English commissioners have recognised the necessity of providing accommodation for the hopeless chronic cases in buildings of a cheaper character than the county asylums. The metropolitan district asylums alluded to may be taken as examples of such erections, and in all probability will serve as models for similar chronic establishments elsewhere. Moreover, the principle recognised in their construction will be fruitful in good results; the principle, namely, of separating to a very large extent the curable from the incurable insane. It is, indeed, but a few years since great names and specious arguments were advanced against any attempt to detach the one class from the other; but experience, as usual, has proved the better teacher, and convinced the well wishers of the insane, of the mischief attending the unlimited herding together in

hundreds of the mentally deranged and mentally deficient of all sorts. The overgrown and under-officered asylums of some counties may indeed be looked upon as manufactories of insanity by depriving the curable insane of their chances of recovery, and so adding to the sum of incurable.

But, to continue, the multiplication of the insane is such that county asylums, and pauper district asylums, and workhouse special wards must be supplemented by further arrangements to secure the welfare of the growing multitude. We have seen that not far short of 15 per cent. are distributed here and there in cottages, residing with relatives or with strangers. The questions arise, how far can such a distribution be deemed a suitable one, and how far can it be made so? At present it is a method of distribution having very slight pretensions to possessing any system or organization that can render it satisfactory. At least, so it is in England and Wales; in Scotland the commissioners have much to say on this matter, and much to report of an encouraging matter, which the reader would do well to examine.

Returning to the figures exhibiting the relative increase of lunacy among paupers and non-paupers, its excess among the former is remarkable and needs elucidation. The English Commissioners do not enter on the subject, but the Scottish have some very pertinent remarks.

The pauperism of England and Wales amounted on the first of January 1871, to 1,085,661 in a population of 22,704,108, equal to 4·78 per cent. The total lunatics, of all sorts, equalled 0·249 per cent. of the whole population, and the proportion of pauper lunatics constituted 0·22 per cent. In other words the private insane were as 0·27 in 1000, or 2·2 in 10,000 of the inhabitants of the kingdom, whilst pauper lunatics attained the ratio of 2·2 per 1000. At the same time the proportion of paupers to the entire population stood at nearly 48 in every 1000. The case thus stands, in every 10,000 people of all ages there are 2·2 insane individuals privately maintained, and 22·2 pauper lunatics, and 480 paupers of all descriptions. Turning to Table VIII it is seen that the per centage of pauper lunatics to the whole of the pauper population, as calculated by the Commissioners, is 4·63 for this same year. Hence, speaking generally, the proportion of insane paupers in the population is more than tenfold that of private cases; and prevails in a twenty-fold degree among themselves greater than in the population at large.

The Scottish Commissioners observe that, with reference to lunacy "in its numerical relations to pauperism and to the population, it is controlled by agencies of a tolerably stable character. The general tendency, however, is towards an

increase. Thus in ten years the proportion of pauper lunatics in the general population has increased from 180 to 202 in every 100,000; and the proportion of lunatics to paupers from 66·57 to 77·14 per 1000. It must, however, be taken into account that in these calculations the population has been taken according to the census of 1861. Consequently, the relative growth of lunacy must be somewhat overstated; but the increase in the proportion of pauper lunatics to paupers rests upon recent figures" (pp. xiv, xv).

From these statements it follows that the ratio of pauper lunacy to the whole population is greater in England than in Scotland: viz.: as 220 to 202; but that the ratio of pauper lunatics to paupers is less in England, viz.: as 4·63 to 7·71. This wide difference, however, as the Scottish report explains, is more apparent than real, on account of the variation in the manner in which the calculations are made in the two countries. "In the English calculations, wives and children are individually enumerated, whereas in Scotland they are reckoned as dependents, and do not appear in the number of registered paupers. But if the Scotch calculations be made according to the English method, it will be found that the proportion of pauper lunatics to paupers is, on an average of five years, 1866-1870, 4684 per 100,000, or 244 in excess of the English rate. This excess is probably due to the fuller returns made in Scotland of patients in private dwellings." By the latest English returns, however, as above quoted, the ratio has become in the course of 1870 almost indentical with that in Scotland, viz.: 4·63 and 4·68 per cent. respectively.

We have already noticed that in England with respect to the distribution of pauper lunatics, 61·25 per cent. are placed in asylums, 24·18 in workhouses, and 14·57 with relatives or others. In Scotland, the per centages were 58·0 in asylums, 18·1 in poorhouses, and 23·7 in private houses. In this last named mode of distribution it is that the greatest difference prevails between the two countries.

The Scottish board remarks on the greatly decreasing ratio of private to pauper patients detained in asylums and lunatic wards. In January 1858, the proportion stood thus, 25 private to 75 pauper; but at the same date in 1871, it was 20 to 80; thus showing that whilst at the former period one fourth of such patients were maintained from private sources, at the latter there were only one fifth.

Using the tables of the English reports to calculate comparative statistics, we find that for every single pauper patient resident in establishments 25,000 to 42,000 (asylums, licensed houses, and workhouses) on the first of January 1858, there

were 17 on the first of January 1871, whereas, with regard to private patients the ratio is 47 at the former date, to 57 at the latter. And the proportion of private to pauper patients was in 1858, one fifth, and in 1871, one seventh. Hence the number of cases made chargeable to the country by reason of insanity is greater than in Scotland.

This growing burden of pauper lunacy is consequently a matter of immense importance politically and socially. There need be some explanation why the ratio of cases maintained by private resources should be progressively decreasing, notwithstanding the advance of the country in wealth. Is the cause to be found in an improved mental condition of the richer classes, and a perpetually advancing proclivity to mental derangement among the poorer members of society? Or if such an interpretation be inadmissible, or only partially admissible, to what other causes is the fact to be referred? That there is less madness among the wealthier classes no shadow of evidence exists; and, without question, they contribute at the present time much the same quotum to the growth of insanity that they did thirteen years since. Likewise, although insanity gradually assumes larger dimensions in the country, there is no distinct evidence that it is more rife among the humbler classes who in sickness need parochial aid, than among their superiors in social position. And the consideration of this matter suggests the operation of other causes in explanation of the constantly decreasing ratio of individuals who are either unable or unwilling to bear the burden of maintaining their insane relatives.

The Scottish Commissioners, by certain observations, cast some light upon this matter; they find that the ratio of recoveries among private and pauper patients is in close accordance with the ratio of admissions; but the removals of unrecovered cases among the former is considerable, and such removals necessarily reduce the relative proportions existing between the two classes, particularly as they, in the great majority of instances, represent the transfer of the lunatics from the self-supporting to the pauper class. Such removals are dictated chiefly by social causes. There is on the part of friends a powerful inducement, particularly if their means are straitened, to remove an insane relative from the private asylum, "as soon as all hope of recovery is passed, or the malady has assumed a manageable form; whereas, when the cost of maintenance is defrayed by the parish, there is no pecuniary motive affecting individuals to prompt removal. On the contrary, as the family of the patient, as well as the inspector of the poor, are relieved of all trouble and responsibility in connection with the case, considerable inducement is held out to leave him in the asylum."

The same inducements prevail to add to the pauper lunacy of England and Wales. People at large are ever ready to transfer the cost of insane relatives from their own private to the public purse, and want but small excuse for so doing. Such an excuse is not seldom found in professed greater confidence in the treatment obtainable in the public than in the private asylum; and a faint whisper of self-interest is sufficiently strong to convince many of the propriety, if not of the necessity, of saving their own pockets. The insane friend so cast off, and made chargeable to the public, frequently soon ceases to be much thought of, and is rarely visited; for small interest is manifested in his welfare now that he is numbered among paupers, and few are the longings to have him returned to his home.

Could a minute inquisition be made into the pecuniary means of many patients detained in county asylums at the public cost, and into those of their relatives legally chargeable with their maintenance, we are convinced that an asylum-delivery of no small number would follow. The large dimensions of most asylums, the little individual knowledge possessed by the medical officers of their inmates, the imperfect and incorrect histories furnished on admission, and collusion to no small extent between interested relatives and poor-law officers, are among the circumstances answerable for the undue preponderance of pauper lunacy in the population.

But there is a further cause for this preponderance, to which the Commissioners for Scotland draw especial attention. The circumstance referred to is the difficulty attending the exit of patients from asylums, as brought about by the inducements felt by relatives to leave patients in asylums, and by the apathy and the unwillingness of the medical superintendents to take on themselves the responsibility of recommending discharges.

Referring to a table exhibiting the movements in asylums during recent years, the Scotch Commissioners write:

"It is difficult to believe that any real necessity has arisen for the detention of 504 pauper lunatics in asylums at 1st January, 1870, above the number of those detained at 1st January, 1868, especially as, in the same period, we find an increase of only five in the number of private patients; on this account, as well as for other reasons, we are led to believe that the increase of pauper lunatics is in great measure due to the artificial fostering of legislation. . . In a considerable degree, however, their increase in asylums is owing to the unwillingness of the superintendents of those establishments to undertake the legitimate responsibilities of their office. This unwillingness, it appears to us, arises in great measure from a misconception of their duties. In a general way, asylum super-

intendents are ready enough to admit that many of the patients under their care do not require the special appliances of an asylum, either for their own welfare or for the safety of themselves or the public; yet, as a rule, they abstain from taking the steps pointed out by the statutes for the removal of such cases from asylums."

The same authorities proceed to cite the statutes alluded to, and it is worth while to quote one of these, for it has no parallel in English lunacy legislation, although it evidently can be most efficiently and usefully applied. It is to the effect that the orders, as given by the sheriffs in Scotland, for the admission and detention of patients in asylums shall, after a given interval, cease and determine. It provides that—

"In no case shall the sheriff's order remain in force longer than the 1st day of January first occurring after the expiry of three years from the date on which it was granted, or than the 1st day of January in each succeeding year, unless the superintendent or medical attendant of the asylum or house in which the lunatic is detained shall, on each of the said first six days of January, or within fourteen clear days immediately preceding, grant and transmit to the Board a certificate, on soul and conscience, according to the form of Schedule A, hereunto annexed, that the detention of the lunatic is necessary and proper, either for his own welfare or the safety of the public."

The cause (the needless detention of many insane in asylums) just referred to, of the multiplication of public pauper inmates, holds good, therefore, in the proportion in which such patients stand to private cases in public asylums—a proportion representing by far the greater part of the whole number, particularly in English establishments. In the same proportion, therefore, it adds to the preponderance of pauper lunacy in the population; and this result would not be at all affected if the recommendations of the Scottish Board were carried out, for these would do no more than bring about a transfer of so many incurable insane from the lists of asylums to those of patients lodging in private dwellings, and not affect, in any appreciable degree, the relative proportion of privately and of publicly maintained lunatics.

But we may go a step further in accounting for the excessive ratio of pauper lunatics at the present time, by calling attention to the comparatively recent development of the public provision for the insane, in any measure commensurate with their requirements. There is no doubt we are still suffering from the consequences of neglect of the insane in past years, and that our asylums are yet somewhat occupied with the gleanings of that bygone period; and it is, again, in no way doubtful that the opening of asylums has brought to light numerous victims of mental unsoundness dispersed unheeded in the general

population, and previously tolerated and nursed by those connected with them; nay, more, the establishment of a new asylum in itself operates as an inducement to hunt out and bring together the weak intellects of the community; and, as a matter of course, such come most largely and most readily from the classes of society which most feel their encumbrance and cost; nor is the circumstance undeserving notice, that a larger proportion of chronic lunatics is likely to arise among those classes, by reason, especially in former times, of the neglect of treatment in the early and curable stages. Lastly, it must be remembered that the mere fact of the occurrence of insanity largely recruits the ranks of those dependent on public assistance. Many professional men, as well as many small tradesmen and employers of labour, whose position in life rests upon their own exertions, must, when mental disorder attacks them and lasts for some time, become pensioners upon the public purse.

The Scottish Commissioners have not lost sight of the inducements held out to those desirous of ridding themselves of burdensome weak-minded relatives by the opening out of asylum accommodation. They refer to Renfrewshire, and notice that, since 1870, the increase of lunacy in that county "seems to have been arrested." Of this phenomenon, they add, the explanation is to be found in the fact that no increase of accommodation for lunatics has been provided. To enlarge an asylum or to build another is to invite inmates, and cause to be sent thereto "many persons of feeble or decayed mental powers, who, under other circumstances, would have been tended at home"—a class of patients representing a considerable proportion of those admitted into asylums. "The temptation of gratuitous treatment, and the saving of home trouble which removal implies," are clearly, therefore, largely operative in filling asylums with incurable cases and multiplying a pauper population.

The foregoing considerations elucidate—in some degree, at least—the fact of the very large relative proportion in which lunacy presents itself in the pauper population of the kingdom; but, after allowing them all due weight, the conviction remains that that proportion is excessive, and beyond what it ought to be, regard being had to the wealth and general prosperity of the country; and that a considerable number of persons do yield to the temptation to obtain gratuitous treatment for their afflicted relatives who could well afford to pay, in whole or in part, for their care and maintenance.

And, on the whole, it is perfectly clear that there is a misuse and abuse of public asylums, in transferring to them cases of

mental weakness and perversity capable of being dealt with elsewhere; and, at the same time, an undesirable severance of those bonds and sympathies that should subsist between members of a family, and a paralysing of those feelings of mutual help and of forbearance which sickness and weakness are calculated to call forth. The transfer to strangers cuts off all such, and supplies, moreover, no adequate substitute. Besides, the mode of life and surrounding conditions of existence in asylums are not only novel, but completely overturn and displace those comprised in the word 'home,' and the continual commingling of a multitude of diseased minds is inimical, not merely to recovery, but to the continued maintenance of the same degree of mental power.

It is worth while also to comment on the fact now well established by experience, that the insane are not such aberrant, exceptional beings as they were of old held to be. They do not necessarily lose their sympathies with things human and humane, or their affection towards relatives and friends; they are amenable to kindness and to deterrent influences, and can take part in numerous matters of diversion and employment. In years gone by neither the profession nor the public were educated in so regarding them; but now medical men generally, we may hope, rightly appraise the condition termed lunacy; and the public at large we may assume to have rid itself of its idle fears of the insane and to have learnt the lesson that the great secret of their management is kindness with steady supervision.

Other important questions are suggested by Tables X and XI (now for the first time inserted in the report of the English Commissioners), exhibiting the number and distribution of the pauper insane, and the ratio in which such lunatics stand to the population in the several counties of England and Wales at large.

A glance at these tables shows great diversity in the proportion of pauper lunatics to the population in the various counties, for the ratio ranges from 4·64 per 1000 in Flintshire to 1·20 in Durham. And the cause of such variation does not at once suggest itself, yet it imperatively calls for investigation. In the 14th Scotch report (p. xix) reference is made to this matter, and it is remarked that, to whatever causes the discrepancies may be due, insanity, "in its numerical relations to pauperism and to the population, is controlled by agencies of a tolerably stable character." What these agencies are, however, no positive attempt is made to elucidate. The discrepant proportion is assigned to differences in the constitution and circumstances of the inhabitants in the several counties, but what this con-

stitution and those circumstances may be is at best very partially displayed in the following quotation:

"As a rule, it may be assumed that there is a greater degree of mental activity in towns than in the country; and to this fact may possibly be ascribed the more frequent *occurrence* of insanity among an urban and busy population than among one which is rural and passive. On the other hand, it must be taken into account that the increase of lunacy is found chiefly among the lower classes of the community, which nowhere display much mental activity, and which are most exposed in cities to influences which destroy health. From this cause the changes among pauper lunatics in the asylums of large towns and of manufacturing districts are much more frequent than in those of pastoral and agricultural districts."

Here we may object that neither the tables of the English nor those of the Scottish Board sanction the general statement that insanity occurs more frequently in urban than in rural populations. On the contrary, the ratio of insane paupers to the population ranges, as a rule, higher in agricultural counties than in those in which manufactures and commerce employ a very large proportion of the population. For instance, in Stafford the ratio per 1000 is 1·75, in Derby 1·83, in Lancaster 1·94, and in York (West Riding) 1·42; whereas in Berks it is 3·15, in Bucks 2·84, in Wilts 3·29, and in the East and North Ridings of Yorkshire respectively 1·90 and 1·85. In the same way, according to calculations we have made, the ratio in Inverness is about double of that in Lanarkshire, in which latter county, too, it falls below that in Aberdeen and in Sutherland. The next following statistical table also enforces the same truth.

The word *occurrence* in the extract quoted is certainly emphasised in italics, as though the Commissioners would contrast that circumstance with the *increase* of insanity, and it may be intended to mean that more new cases of insanity originate among the dwellers in towns than among an equal number of those in country districts. If it be so intended, we yet find no evidence of the truth of the assertion, and are disposed to assign it any considerable part in explaining the discrepancies in the ratios in question. Further, from the same point of view our estimate of the remarks relative to the greater number of changes "among pauper lunatics in the asylums of large towns and of manufacturing districts . . than in those of pastoral and agricultural districts" is not very high. Such changes are no sufficient proofs of a greater prevalence of insanity where they may happen to occur, for we know them to be dictated by many casual circumstances, some of which have been noted in a

former part of this paper, whilst others will on reflexion occur to the mind of the reader.

The question, therefore, remains unsolved, to what cause the discrepant ratio in various counties is due. The first thought in explanation that arises is, whether a direct relation exists between the amount of pauperism and the extent of lunacy. That insanity pauperises, and that the ratio of lunatics chargeable as paupers very far surpasses that of the prevalence of insanity in the population at large, has been already sufficiently insisted on; but we wish now to examine the ratio of pauperism in certain counties, and the relations obtaining between such pauperism and the diffusion of insanity therein, noting at the same time the principal characters of those counties in connexion with the occupation of the inhabitants. The following table which we have drawn up for the purpose sets forth the chief points in question:

	Ratio of paupers to population per 1000.	Ratio of pauper lunatics to 1000 in the population.	Ratio of pauper lunatics to paupers per 1000.
Metropolis, including parts of Middlesex, Kent, and Surrey	51·3	2·02	39·4
Middlesex (county) (as given by the Commissioners)	51·4	2·86	55·7
Lancaster	31·9	1·94	61·1
Stafford	37·1	1·44	38·7
York (West Riding)	30·7	1·42	46·2
„ (North Riding)	34·9	1·85	53·1
Derby	28·7	1·83	63·9
Devon	58·1	2·37	40·8
Dorset	78·9	2·58	32·7
Bedford	69·5	2·48	35·7
Hereford	55·0	3·42	62·2
Berks	63·5	3·15	49·6
Lincoln	51·0	2·02	39·6
Westmoreland	32·7	2·01	61·3
Cardigan	69·0	1·61	23·3
Glamorgan	48·9	1·69	34·5
Flint	65·9	4·64	70·3

Now, a glance at this table shows that, in a certain degree, a direct ratio does subsist between the extent of mental disorder in a county and the prevalence of pauperism. In each of the counties enumerated having 50 paupers in every 1000 of the population there are, in the same number, 2 to 4·64 lunatics, with the exception of the one Welsh county, Cardigan, where the proportion of lunacy in 1000 inhabitants is only 1·61. As a set-off against this Welsh exception, we have the case of Flintshire, whose inhabitants would seem to have very soft heads, for they produce not far short of 5 lunatic paupers in every thousand of their number, and therein exceed the ratio in all other counties.

A further examination will, however, indicate that the proportion of lunacy stands in no exact relation to that of pauperism. For instance, Dorset, which has unfortunately the pre-eminence as a pauper-abounding county, has fewer lunatics in proportion than others, such as Bedford, Hereford, Berks, and Middlesex, in which the pauper class are in a much smaller ratio.

Proceeding a step further, and taking into account the next column, exhibiting the ratio of pauper lunatics to paupers, it becomes evident that there are some circumstances which exert no mean influence on the prevalence of insanity in a population; for this column demonstrates that the ratio of lunacy among paupers may differ greatly, even where the ratio of lunatics in the general population is closely similar. In other words, insanity appears much more abundant among paupers of some counties than of others; compare, for example, the figures exhibiting those relations in the counties of Lincoln, Westmoreland, Lancaster, and Middlesex.

This fact, doubtless, is attributable to many causes, of which a considerable number are difficult of recognition. The first to suggest itself is, the different modes of life and occupation in the several counties. Some are pre-eminently agricultural, and others manufacturing; and the preceding table seems to confirm, in a general way, the remark before made, that madness is more rife among the paupers in agricultural than in manufacturing counties.

In the column under notice, as in the other one alluded to, Flint heads the list, and is followed at no great distance by Derby and Hereford, and then by Lancaster and Westmoreland. Regard being had to these counties alone, the broad statement just made does not hold good; for Lancaster may be taken as a special instance of a county in which manufactures and commerce preponderate, and the major part of the inhabitants are urban; whilst Hereford and Westmoreland might be quoted as good examples of agricultural counties and population. Derby occupies an intermediate position between the two examples, as it possesses a considerable population engaged in manufactures and in mining. Moreover, with respect to the self-same counties, the comparative rate of pauperism throws no light on the fact of the large per-centage of lunacy among their pauper population; for Derby is lowest on the list, and Lancaster and Westmoreland stand immediately above; whilst, again, Dorset, which has an amount of pauperism double that of the counties above enumerated, except Hereford and Flint,—though these two likewise fall considerably short of that amount, presents little more than half the insanity among its needy classes that they do.

It is, therefore, perfectly clear that, besides pauperism and the conditions of agricultural existence, there must be some potent agencies at work among the poorer classes in Lancaster, Derby, and Flint, to give origin to so high a ratio of lunacy among them. To endeavour to trace out those agencies, and to assign them their relative value, would involve us in details and discussions that would require much more space than can be given in the present article. We will turn, therefore, to some other portions of the Lunacy Reports before us that call for remark, but we shall have to dismiss them with fewer observations than their importance would demand.

From some extracts already made from the report of the Scotch Board, it will have been seen how decidedly the Commissioners have set their face against the prevailing tendency to burden asylums with hopeless and harmless cases. They distinctly recognise the impracticability of meeting the demands for asylum accommodation should the present system of aggregating all classes of insane people be persevered in, and they call for an asylum-delivery of helpless and harmless chronic cases. They put the straightforward question, "Whether the constant expansion of the asylum system is not detrimental to further progress in the rational treatment of insanity," since "the more the patients are increased in number, the less powerful becomes the moral influence to which they are subjected. The superintendent ceases to individualise the different cases, and time and chance have generally far more to do with the result than any special treatment which he may have ordered" ('Report,' xiii, p. 44). They further point out that where chronic cases accumulate in large numbers, this circumstance must affect injuriously the professional position of the medical superintendent, and augment his difficulties in training attendants and efficiently supervising their work.

The English commissioners are not so outspoken in their opinions; they rather follow the avowed principle of her Majesty's Ministers, and await the manifestation and growth of opinion; not initiating views, but holding themselves in readiness to assume the direction and control of such as may make good their claim to notice. However, their reports show that there is a general harmony of opinion between them and the Scottish Commissioners, and that they deprecate the perpetual extension of asylums and the indiscriminate relegation of mentally disturbed people, of all sorts and kinds, to one and the same description of establishment.

Further, they are anxious—and very properly so—to secure for the inmates of asylums more complete medical supervision than is their lot in several such institutions; but they have not

committed themselves to such comparatively heterodox views as are enunciated by the Scottish Board. The latter are impressed with the deteriorating effects on the superintendent of asylums, considered as a medical man, when those establishments are filled almost entirely with incurable cases; and that officer has to abdicate his medical position in favour of one rather recalling that of a steward and manager of a large boarding-house. In such institutions, they remark that the demand for real medical attainments is reduced to a minimum, and that administrative tact and a knowledge of agriculture are more valuable qualifications. "Modern asylums," they proceed to say, "have, in fact, assumed a great deal of the character of boarding-houses, in which the general well-being of the patients is far more dependent on comfortable meals and beds, and on adequate exercise, occupation, and recreation, than on any special medical treatment."

There is much truth in these representations of the Scottish Commissioners; but, although this is the first time such statements have been officially put forth, they really reflect a very general persuasion pervading the minds of visiting justices for many years, and influencing them largely in the choice of candidates for asylum appointments. In fact, no magistrate or other non-medical visitor of our large asylums can fail to see how large a part administrative tact, well-devised rules of management, and economical principles, play in their direction, and how little scope can be discerned for the exercise of medical talent.

There are some admirable observations on the mortality of asylums, as compared with one another and with the general mortality of the population, contained in the just-issued fourteenth report of the Scottish Commission, to which we can now only draw attention.

Statistics show that the mortality in English asylums is about a fifth higher than in Scotch asylums. A possible cause of this difference is suggested in the different dietary of the two countries, but it is remarked that "it may be entirely dependent on the more severe forms of insanity, or in the more serious complications of bodily disease, with which the patients are affected." We are disposed to add another circumstance in explanation of the difference; to wit, a larger proportion of old and broken-down inmates in English institutions, the residue accumulated during a longer series of years than the Scottish asylums generally have been in operation.

With regard to the comparative mortality in asylums, and in the general population, the reporters rightly point out that no comparison can be properly made between the two, the circum-

stances obtaining with them are so entirely different. This fact they go on to illustrate, and for the purpose select the mortality from pulmonary consumption. This disease, as calculated per cent. in asylum inmates and in the population at large, appears to prevail equally in the two; but such a conclusion is fallacious. "This will be at once apparent when it is kept in view that the mortality of 17·3 per cent. (on the total number of deaths) from consumption in asylums takes place among a class selected on account of diseased mental manifestations, and that this selection involves an abnormally high mortality from lesion of the nervous system (viz. 34·9). If, then, among the remaining patients the deaths from consumption are still sufficiently numerous to give a per-centage of deaths surpassing that from the same cause among the general community, it necessarily follows that consumption must occur in a much higher ratio among the insane than among the sane."

A still more material element in the matter is the age of the communities between which the comparison is made. In the one there are very few individuals under the age of puberty, in the other persons of all ages occur; hence comparison between so unlike groups is not legitimate.

There is yet another point to be remembered. "The annual mortality of asylums is 8·2 per cent. on the number resident. The mortality of the general population, however, is only about 2·1 per cent., or a fourth of that of the asylum population." Consequently, even if it be assumed that the proportion of deaths from consumption to the whole mortality be alike in the two communities, "there would still occur among the latter four times the number of deaths from consumption which occur among the former, simply from the higher rate at which, as a class, asylum patients die. That is, population for population, four times as many deaths as occur from consumption among the insane would happen among the sane" (p. xxx).

The English lunacy reports contain, as unfortunately is customary, references to various distressing accidents in asylums, and detail the painstaking proceedings of the Commissioners in investigating those occurrences, and conducting prosecutions of those concerned in them. The result of their inquiries is generally the detection of neglect, of violence, or of want of proper appliances and regulations; and although the law seconds their efforts to punish the guilty, and their own authority enforces rigid rules to guard against negligence and to supply deficiencies, yet the annual crop of serious accidents and injuries appears to suffer no positive diminution.

Now, it appears that, speaking generally, there is a direct relation between the number and severity of accidents and the

size of an asylum; and, in connection with this circumstance, we gladly quote it as the opinion of the English board, "that a gradual enlargement of asylum buildings, far beyond the limits of personal supervision or individual control, has removed the only safe guarantee against those temptations to the exercise of power by which the best attendants are corrupted, and to which the average of those now employed are especially liable" ('Rep.' 24, p. 73).

And the Commissioners proceed to point out the necessity, on the part of the superior officers, for the greatest vigilance, and "of severity of supervision to the utmost possible degree," of attendants. For, as they observe, nothing is easier for a man in the position of an attendant, "with unrestricted and uncontrolled power over the habits and happiness of another, than to act cruelly without being cruel. He has but to neglect and be careless of his patient, which, if not conscious himself of a vigilant superior above him, it is more than probable he will be. He has but to use freely the powers in his hands to excuse or conceal his own idleness."

The Commissioners for Scotland follow in the same track and animadvert on the evils accruing to attendants by overgrown asylums. In every institution for the treatment of the insane the physician "has, in a great degree, to rely on the zeal, the attention, and the kindness of his attendants. Attendants, however, are frequently not trustworthy, and are occasionally even guilty of harshness and cruelty. Nor is this surprising. The life of an asylum attendant is one which presents few attractions, and its rewards are inconsiderable. Consequently there are frequent changes in almost every asylum staff. Many dislike the work, or their health suffers, and they leave after a short trial. Others are soon discharged for incapacity, inattention, drunkenness, insubordination, cruelty, or some similar cause." These changes, it is worth while to note, are more frequent in larger asylums, and in the Scottish establishments amount annually, on an average, to considerably more than a third of the staff. Another circumstance is that not only do they enter upon duties respecting which they are profoundly ignorant, but it is too frequently the case that no special care is taken to teach them. They are left to pick up for themselves what information they can, and this almost wholly from colleagues in the wards. Consequently, with a modicum of acquaintance with the rules propounded officially for their guidance, they become initiated also into the traditions and usages of fellow-attendants of older standing, and not a few of those, to say the least, of doubtful character and expediency. And such traditions and usages rank as mysteries of their office, and concur with the conditions of com-

mon life and the sense of common interest to knit together attendants in a special craft, whose members owe to each other mutual support, forbearance, and secrecy.

Admitting the truth of the foregoing remarks, less mystery will be apparent both in the character of many of the accidents reported, and in the difficulties surrounding investigations into their cause and the detection of the parties involved in their occurrence.

Both the English and the Scottish Commissioners, impressed with the responsible position of attendants, and with the sad history of neglect and cruelty in asylums, have sought to elevate the position of attendants and to provide against the hiring of inefficient and improper ones. They endeavour to attain these ends by holding out inducements to a better educated and higher class of applicants, and by instituting a system of registration, setting forth the grounds of discharge or dismissal. What they have done in this direction is narrated by the English Commissioners in their 24th Report, to which we would refer the reader.

And here we must bring to a close this review of the contents of the reports under notice, not from lack of material, but from lack of space; for several important themes, and among them the state of the insane in workhouses, the extent of the use of mechanical restraint, and the success of the treatment of lunatics in cottage homes, would furnish ample matter for profitable discussion.

J. T. ARLIDGE.

VIII.—The Present State of Cardiac Diagnosis and Therapeutics.[1]

ANDRAL, in editing the fourth edition of Laennec's 'Traité de l'Auscultation Médiate et des Maladies des Poumons et du Cœur,' in the year 1837, writes in a note, "In spite of many laborious researches, the history of the diseases of the heart is far from being yet finished; it still presents a great many doubts

[1] 1. *A Practical Treatise on the Diagnosis, Pathology, and Treatment of Diseases of the Heart.* By AUSTIN FLINT, M.D., Professor of the Principles and Practice of Medicine and of Clinical Medicine in the Bellevue Hospital Medical College, &c. Second Edition. pp. 550. Philadelphia, 1870.

2. *Lectures on the Principles and Practice of Physic, delivered at King's College, London.* By Sir THOMAS WATSON, Bart., M.D., F.R.S. Fifth Edition. 1871.

3. *Oppolzer's Vorlesungen. Die Krankheiten des Herzens.* pp. 111. Erlangen, 1866.

to be removed, and many gaps to be filled up, and the time is not yet arrived when the diagnosis of these diseases can be regarded as being so easy and certain as that of the diseases of the lungs. Still, the continual progress which has been made from the time of Lancisi to that of Laennec, and from the latter to that of M. Bouillaud, must inspire us with hope for the future." If to these remarks we add, "and from the time of Bouillaud, Hope, and C. J. B. Williams to the present day," the remarks of Andral are still applicable, for it is quite true that the whole history of the diseases of the heart is even yet, in the year 1872, not completed, and that some of them are still enveloped in a certain amount of obscurity; that the relations existing between the rational and physical symptoms during life and the appearances found after death are not yet absolutely determined in all cases; and that very many problems of treatment yet remain to be solved. Still, much more has been done in all these directions than Andral, in the preface referred to, appears willing to admit, and it may be asserted with tolerable truth that the diagnosis of cardiac diseases is at least as easy and certain as that of the lungs, while in reference to therapeutics there can be no doubt that a very great advance has been effected, not only by the introduction of new or improved remedial measures, but by the omission of many old ones which are now proved to be mischievous.

Andral, while he attributes great merit to the labours of Bouillaud, does very little justice to the British investigators of cardiac diseases, especially the late Dr. Hope and Dr. C. J. B. Williams, although their researches were made before the period when Andral wrote the preface to Laennec's treatise. In fact, the elaborate and conclusive experiments made on the causes of the sounds of the heart by Drs. Hope and Williams, by the Dublin committee of 1835, and by the London committee of 1836, have thrown a complete flood of light upon the diagnosis of diseases of the organ, and have almost cast into the shade the brilliant investigations of Laennec himself, so far as cardiac auscultation is concerned. For the great French stethoscopist, while he explained very clearly the auscultatory phenomena of the rest of the chest, left the præcordial region in a great measure to the labours of subsequent explorers. Although, too, Laennec indicated the character of the sounds caused by the motions of the heart, he made no attempt to explain the mechanism by which they were produced; and while to him, moreover, is justly due the discovery of the abnormal cardiac sounds or murmurs (*bruit de soufflet, bruit de râpe,* &c.), he was unable to account for their occurrence except upon the supposition that they denoted a spasm of the heart. "It appears to me," he

says, in treating of this subject in his 'Treatise on Mediate Auscultation,' "that the positive and negative facts which I have just adduced all tend to prove that the *bruit de soufflet* is produced by a simple spasm, and does not indicate any lesion of the heart or arteries." He was, however, very near to the discovery of the valvular origin of the murmurs when he states that "the *bruit de soufflet* exists pretty constantly in the heart of persons who are affected with narrowing of the orifices of this organ;" but he goes on somewhat erroneously to state that "it is also pretty frequently met with in persons affected with hypertrophy or dilatation; but it is found still more frequently, as well in the heart as in the arteries, of persons who have no lesion of these organs, and who are attacked by very different affections."

The progress made in our knowledge and practice in reference to cardiac diseases in the present day is still more strikingly displayed by the perusal of the single short chapter devoted by Laennec to the treatment of the organic affections of the heart. "Of all the organic affections of the heart," he writes, "simple hypertrophy or combined with dilatation appears to me the most susceptible of cure. By applying courageously and perseveringly to the treatment of hypertrophy the method advised by Valsalva and Albertini for aneurism of the arteries, we may promise to ourselves much more frequent and complete success, especially if we begin to employ it at a period when the disease has not yet produced serious general symptoms. . . The treatment ought to be pursued in an energetic manner, especially at first. . . We must begin, therefore, by bleeding the patient as copiously as he can bear without fainting, and the bleeding must be repeated every two, four, or eight days, at the latest, until the palpitations have ceased, and the heart no longer gives more than a moderate impulse under the stethoscope."

Laennec goes on to advise that the patient must reduce his diet by at least one half, and *after about two months of bleeding and low diet*, if he no longer suffers from palpitation, or presents a strong cardiac impulse, then the bleeding may be practised at more distant intervals, and the *severity of the regimen may be in some measure relaxed.* But he adds that the same measures must be resorted to, and with equal vigour, if the impulse of the heart should afterwards increase.

The influence of these doctrines and of those of Bouillaud, who carried out the system of sanguineous depletions to a still greater extent, especially in recent cardiac affections, was, no doubt, exceedingly injurious, and the success said to attend such treatment must rather be attributed to the enthusiastic and coloured statements of the practitioners of the period than to any actual relief afforded to the patient. But the inutility of

bleeding in the chronic organic diseases of the heart, and its questionable efficacy even in the acute ones, were among the therapeutical doctrines which made but tardy progress even in this country, and we have only to turn over the pages of some of our most distinguished writers to find abundant confirmation of this proposition.

Dr. Hope, whose classical 'Treatise on Diseases of the Heart' has been of invaluable service to the progress of cardiac pathology and diagnosis, very distinctly condemns the treatment recommended by Laennec, but he admits that he himself has invariably found the greatest benefit to be derived, in the early stages of hypertrophy of the heart, from sparing abstractions of blood at intervals of two or three weeks or more. His plan of treating hypertrophy of the heart consisted in the abstraction of blood to the amount of four, six, or eight ounces, every two, three, four, or six weeks, according to the age and strength of the patient, so as merely to keep down palpitation, dyspnœa, and strong impulse of the heart. But although this plan was recommended, and no doubt adopted, only about twenty years ago, it is now justly discarded, as not only useless but mischievous, for experience has shown that its tendency is to continue or to aggravate the very symptoms which it is intended to relieve, while the hypertrophy itself, so far from being a specific morbid condition, is rather to be regarded as a compensatory provision of nature to assist the walls of the heart in carrying on the circulation in spite of the mechanical obstacles opposed to the due performance of that function.

But while Dr. Hope recommends the cautious abstraction of blood in hypertrophy of the heart, he is by no means sparing of this mode of depletion in the acute affections of that organ. In the treatment of acute pericarditis and endocarditis he advises that the antiphlogistic treatment, in as energetic a form as circumstances will allow, should be employed with the utmost promptitude. If the attack is recent, and the patient's strength will admit, he recommends that blood should, in the first place, be drawn freely from a large incision in the arm of the patient, in the erect position, so as to bring him to the verge of syncope. From five-and-twenty to forty leeches, according to the strength, should then be applied to the præcordial region as soon as the faintness from the venesection disappears and reaction commences.

Bouillaud, writing in 1835, announces as a happy discovery his method of bleeding copiously and frequently in pericarditis, and he declares that this kind of treatment has been so successful in his hands that recovery has become the rule and death the exception in such cases; while, according to his prede-

cessor and fellow-countryman, Corvisart, death was the rule and recovery the exception. "As a general rule," Bouillaud writes in his chapter on the treatment of pericarditis in his 'Traité Clinique des Maladies du Cœur,' "in a subject in the prime of life, three or four bleedings from the arm, in the first three or four days, assisted by the application of twenty-five to thirty leeches, or of cupping-glasses, repeated two or three times, will be sufficient to cure the disease."

There is no physician of our age and country whose opinions have justly commanded more respect and deference in relation to diseases of the heart than Dr. Latham, and in his deeply thoughtful and excellent lectures on subjects connected with clinical medicine he thus discourses on the treatment of acute cardiac disease:

"Bloodletting and mercury and opium are your remedies for these diseases of the heart. . . . Bleeding, mercury, opium—the very remedies you used in acute rheumatism—are, I say, still your main reliance when inflammation attacks the heart. . . . With respect to venesection, if in acute rheumatism any of those symptoms referable to the heart are present which have been already mentioned, auscultatory or non-auscultatory, and especially if they have arisen under your own observation, or, though not under your own observation, if they be now present and you have reason to believe that they have *recently* arisen, then, should the pulse be found to have even a notable degree of that hardness which is deemed inflammatory, blood must be taken from the arm. . . . There is greater hazard in omitting it wrongfully than in practising it wrongfully."

This advice was offered to the profession in 1845, and it would be easy to quote opinions almost exactly similar in their nature from the writings of most, if not all, the clinical teachers of that and preceding periods, and the same doctrines were taught and acted upon in the medical schools of London and elsewhere within the last twenty years. At present, as is well known, not only is bleeding entirely abandoned by most British practitioners in the chronic diseases of the heart, but the practice is considered questionable even in the acute. Perhaps, however, the complete abandonment of sanguineous depletion as a remedy in acute cardiac affections has been carried too far, and a revulsion of feeling on this subject is already beginning to manifest itself.

The change of opinion as to the treatment of the acute diseases of the heart may be very well traced in examining the writings of the late Dr. Todd, whose clinical teachings, opposed as they often were to the views generally entertained by his contemporaries, were always marked by boldness, independence, and originality, although it must be admitted that some

of his therapeutical doctrines were pushed too far. It was, no doubt, often useless, if not injurious, to draw away large quantities of blood, in the hope of curing acute rheumatism, or of obviating inflammation of the cardiac membranes; but it by no means follows, therefore, that *all* inflammatory and congestive diseases, and all fevers, should be treated by the administration of large doses of French brandy. Yet it may be stated, without much exaggeration, that such was Dr. Todd's practice, especially in the later years of his life.

In his Gulstonian Lectures, delivered in 1843, this physician recommends that the utmost vigilance should be exercised in detecting the first indications of acute cardiac disease, in cases of acute rheumatism; and if any abnormal sounds should be discovered, then the practitioner should have recourse to local bleeding over the cardiac region. He recommends that a large number of leeches should be at once applied, or a good quantity of blood should be taken by cupping. He has no doubt, he says, that by prompt practice of this kind, inflammation of the heart may be checked at its commencement, and that the future comfort of the patient depends on the vigilance and sagacity of his medical attendant at this conjuncture. Dr. Todd immediately adds that his experience leads him to value very lightly the efficacy of general bleeding in inflammation of the heart, and he says that he has never seen an instance in which it unquestionably did good.

In a subsequent course of Clinical Lectures on Certain Acute Diseases, published in 1860, Dr. Todd goes a step further, and condemns the abstraction of blood altogether. In this work he remarks that two objects are to be kept in view in the treatment of cardiac affections accompanying rheumatism, namely, to check the morbid process completely, or to restrain it from producing such changes as may prove destructive to the heart; and, secondly, to obviate liquid effusions, which may distend the pericardium and compress the heart, and so become seriously detrimental to life. With regard to the first point, Dr. Todd thinks it is clear that bleeding has no power to stop or prevent the formation of the fibrinous concretions which are apt to collect on the valves. He asks the question whether bleeding will alter the state of the blood which is favourable to the formation of plastic deposits, and he answers it in the negative. Bleeding, in fact, as is proved by experiments, considerably diminishes the red particles, augments very much the proportion of water, and affects the fibrine very little. He quotes Dr. Christison, who has shown that the amount of fibrine increased under bleeding; and also Dr. Beale, who showed the same fact in the blood of a dog, bled on four successive days.

Dr. Todd, as the result of his later reflections and experience, concluded that the practice of bleeding, whether local or general, was objectionable in the treatment of the cardiac affections associated with rheumatic fever; that such depletion was altogether unsatisfactory in its remedial results, and prejudicial in its consequences; and that the abstention from this mode of treatment was perfectly safe, and tended to the best results.

In examining the books still more recently published on the treatment of acute cardiac diseases, and in observing the practice of the present day, we find that the system of treatment by sanguineous depletion is almost entirely abandoned; and even those who were the warmest advocates of this measure have for the most part admitted that their former practice and teaching were erroneous. Sir Thomas Watson, whose recently published edition of his well-known lectures contains the latest results of his long experience and matured reflection, candidly admits that the beneficial results which he formerly expected in acute cardiac disease from the adoption of bleeding were illusory; and while he now expresses his own disbelief in the remedial agency of bleeding and mercury, he refers with what he calls "a melancholy sort of satisfaction" to the opinion now entertained by his distinguished friend, Dr. Latham, that a complete cure of inflammation can no longer be expected from these measures. But Sir Thomas Watson does not discard bleeding altogether in the treatment of acute cardiac disease; and he thinks that at the onset of pericarditis it may do good, but that when the *to-and-fro* sound has manifested itself — that is, when the inflammation has gone as far as the effusion of coagulable lymph—then, if the patient do not die outright, there will be adherent pericardium, and he will survive only for a time.

Oppolzer, who may perhaps be regarded as the exponent of the modern German views on this subject, does not condemn bleeding in the treatment of the acute diseases of the heart, but recommends that it should be employed with caution. In pericarditis he advises its adoption in combination with the use of quinine, more especially when, together with weakness of the heart's contractions, there are indications of congestion in the brain and lungs. He thinks, too, that bleeding is especially indicated in pericarditis, when the fever is high, and the patient is strong and young, and otherwise healthy; and when he complains of violent pains in the breast and head, or of tightness of the chest, or difficulty of breathing. Bleeding, he considers, is also advisable when there is hyperæmia of the brain or lungs, but not when there is irregularity of the respiration, or alternate diminution and irregularity of the pulse.

While Sir Thomas Watson mayfairlybe regarded as expressing the most recent views of practice, in reference to the treatment of cardiac disease in Great Britain, and Oppolzer may be considered an equal authority in Germany, Dr. Austin Flint gives the results of his own treatment, and those of his countrymen in the United States. The last-named writer, while alluding to the practice of Bouillaud and Hope in the employment of blood-letting; and to that of Stokes and Todd, in abstaining from it, adopts a middle course in reference to this powerful agent. He thinks that its indiscriminate use in cardiac, as in other inflammations, cannot but be productive of great harm; whereas, when judiciously employed, it may in certain cases do good. A person in fair health and vigour attacked with acute pericarditis may be a proper subject for blood-letting at the onset of the disease. Resorted to under these circumstances, Dr. Flint considers that it will not cut short the disease, nor perhaps abridge its duration, but it may contribute to diminish the intensity of the inflammation, and thus, besides affording immediate relief, may lessen the prospective dangers of the disease. These views were propounded by Dr. Flint, in the first edition of his work on 'Diseases of the Heart,' and they are essentially the same as those he has advanced in the second, which has recently appeared; but, he adds, that during the ten years which have elapsed he has had no experience as regards either the good or the bad results of bleeding in pericarditis, for in none of the cases he has observed during this period has this measure been employed.

In reference to other therapeutical measures once supposed to be efficacious in the treatment of cardiac diseases, both acute and chronic, it must be admitted that much distrust and uncertainty now exist. Mercury, which was in comparatively recent years strongly recommended in the acute affections, is at present pretty generally discarded as valueless, and it is proved on good authority to increase very often the symptoms which it is given to alleviate. Dr. Flint's experience of the use of this drug during ten years has been very limited, but in two cases in which he observed the results of its employment, the inflammatory symptoms continued, or were actually aggravated, under its use. Sir Thomas Watson expresses a similar distrust, founded on his own recent experience, and on the practice of other British physicians.

But, leaving for awhile the domain of cardiac therapeutics, it is satisfactory to turn to the triumphs which have been achieved in the exact diagnosis of the seat and nature of the diseases of the heart, although it must be admitted that our knowledge of even these matters is still in a transition state. The negative evidence

which may now be deduced, with tolerable certainty, as to the absence of cardiac disease, although the general symptoms may point in that direction, is of inestimable value; and even when disease is proved to exist, the morbid indications differ very widely in their pathological importance, and some conditions, once supposed to be morbid, can, with our present knowledge, hardly be considered morbid at all. Hypertrophy of the heart, for instance, which formerly seemed to demand the care of the physician for its relief, is now shown to be in most, if not in all cases, a compensatory provision of nature to obviate the difficulties of the circulation caused by obstructed or insufficient valves; and to diminish the overgrowth, even if we were able to accomplish such an object, would be to frustrate a beneficent operation of Providence. The diseases of the valves, also, although undoubtedly dangerous, are not all equally so, and the accurate diagnosis which is now attainable may indicate sources of comfort where formerly there was nothing but despair.

But perhaps the most satisfactory result of the knowledge of cardiac diseases possessed by the physicians of the present day, is the growing conviction that these affections are not nearly so fatal as they were once supposed to be. Corvisart adopted as the motto for his book on Heart Diseases, *hæret lateri lethalis arundo*, and Sir Thomas Watson in the last edition of his lectures still somewhat overrates the danger of these maladies. In point of fact, many of the chronic diseases of the heart are borne by the patients with such little discomfort that they are even unconscious of their existence, and in the case of the acute diseases of the organ, a fatal result is now the exception rather than the rule. It may almost be said—although the admission is somewhat discreditable to our art—that, where an unfavourable termination is observed in an acute cardiac inflammation, the failure is probably due to over-medication rather than to supineness on the part of the practitioner. Speaking with due reservation as to the medical care required, it is not too much to allege that the *vis medicatrix naturæ* asserts its supremacy even in the successful issue of an attack of acute cardiac inflammation; while the adoption of large bleedings, and the administration of mercury and purgatives in such cases, are measures which have never been proved to accomplish any good purpose, and may do great harm. Assuming that the inflammation runs its course and that lymph is thrown out both on the outside and inside of the heart, these results are not incompatible with the after enjoyment of fair health in very many circumstances, as has been abundantly proved. Adherent pericardium in the one case and thickened valves in the other, will in all probability

supervene, but the danger of these conditions has been much exaggerated.

It is a trite observation that our present knowledge of the exact seat and nature of heart-diseases is altogether due to the application of the stethoscope or rather of mediate auscultation, and, indeed, without such aid the diseases in question would in most case not be detected at all. At the beginning of the present century the knowledge of the existence of heart-disease was generally acquired only after the death of the patient, and the phenomena observed during life, if they were recorded at all, were not referred to their true causes. But even after the brilliant discoveries of Laennec, the true pathological significance of the morbid sounds of the heart was for a long time imperfectly appreciated.

The labours and researches of Dr. Hope and Dr. C. J. B. Williams in this country, and of Bouillaud in France, perhaps contributed, more than any others, to the diffusion of correct diagnostic views in connexion with those morbid sounds which had originally been described, but were not sufficiently explained by Laennec, and the first result of this newly acquired knowledge on the part of the medical profession was the detection of many cases of heart disease which had previously been either mistaken or unsuspected. But as heart-disease in any form was then generally regarded as a most dangerous affection and almost necessarily fatal, a great amount of alarm was excited in the public mind, and the injury thus occasioned almost counterbalanced the benefits which the revelations of the stethoscope and the successful researches of physicians had conferred upon mankind. In some cases, indeed, the knowledge acquired by the stethoscope led to the very consequences which medical science is intended to avert, and a needless amount of alarm and suffering was inflicted on susceptible patients, who were unaware of being the subjects of any serious ailment, and whose life was embittered by their apprehensions, although they might perhaps have suffered nothing from their disease. For more recent and matured experience has shown that diseases of the heart, both acute and chronic, are far less fatal than was generally supposed, and that persons afflicted with such diseases may be unconscious of their presence and may reach the ordinary term of human existence. Nay more, it may be assumed that a consciousness of heart-disease on the part of the patient is in the inverse ratio to its real existence, and that while the sufferers from merely nervous affections of the organ are acutely sensitive to its actions, those who are the victims of the more serious maladies are quite unconscious of its derangements or of the peril which thereby threatens their existence.

The forms of heart-disease which are usually and perhaps correctly described as the most common causes of sudden or early death, are hypertrophy and dilatation, diseases of the valves, and fatty degeneration. These affections, and more especially the last, are often so insidious in their invasion and progress, and sometimes cause so little inconvenience, that they escape notice or detection during life, and their existence is revealed only by post-mortem examination. But not only is there this comparative immunity from suffering in those cases which steal on in a gradual and imperceptible manner, as frequently happens especially in persons advancing in years, but even the more severe and well-marked forms of valvular and other cardiac diseases are often quite compatible with the enjoyment of moderate health and the performance of the ordinary duties of life.

Dr. Flint very justly remarks that an important part of the management of the cases of valvular, and he might add, of other diseases of the heart, relates to the communications on the subject to be made to patients. An endocardial organic murmur which is discovered incidentally in the examination of a patient need not be announced, for the danger may be prospective and perhaps remote, and gratuitous uneasiness may be inflicted upon the patient. Dr. Flint adds that the lesions which give rise to murmurs are often innocuous, and even when they are of a nature to involve obstruction or regurgitation, and have led to considerable enlargement of the heart, life and comfortable health may be preserved for many years. Moreover, statistics show that sudden death is by no means so frequently a result of heart-disease as has been supposed, that event being very often due to some associated morbid condition, such as structural degeneration of the cerebral arteries, leading to rupture and extravasation. Still we agree with Dr. Flint that while it is unnecessary to give needless alarm to the patient, the prognosis delivered to the friends should be cautious, because the duration of life in cases of organic cardiac disease is very variable. On the one hand, there is a liability to intercurrent affections which may unexpectedly prove fatal; and on the other hand, patients often live for a long time after the symptoms denote lesions of a very serious character.

But even when the existence of valvular disease of the heart is fully ascertained, the prognosis may very materially depend, in reference to the relative danger, upon the precise seat of the lesion, for all the valvular lesions are not equally inimical to life. It is, however, very difficult to determine, with anything like certainty, the comparative importance of the respective valvular lesions, and the more especially as the diagnosis of

these affections has lately undergone some very important changes, as will shortly be noticed. Dr. Peacock, in a very recent contribution (1871) to the 'St. Thomas's Hospital Reports,' availing himself, of course, of the latest improvements in diagnosis, conceives that incompetency of the aortic valves is more dangerous than a similar condition of the mitral valve, but on the other hand, that obstructive disease of the mitral valve is a more important defect than the same affection of the aortic valves. The order of the relative danger in valvular disease, beginning with the most serious affections, would therefore be, according to Dr. Peacock, as follows:—Aortic regurgitant disease; mitral regurgitant disease; mitral obstructive disease; aortic obstructive disease. Dr. Peacock gives valid reasons for placing the respective lesions in this order, but experience is, of course, still wanting either to confirm or invalidate his views.

The object of the present notice being to record the progress of our knowledge of cardiac disease, and to show the particulars wherein the theory or practice of our own day differs from that of our predecessors, or even from our own in former years, it is unnecessary to advert to the diagnostic signs by which the several cardiac lesions are distinguished from one another; at least, in so far as the doctrines of Bouillaud, of Hope, of Walshe, and other distinguished writers on cardiac pathology, remain uncontested canons for our guidance. But the recent changes which have been wrought in the diagnosis of the affections of the mitral valve are too important to be passed over without special notice.

It is well known that the mitral valve may present an obstruction to the current of the blood, and may also allow regurgitation in consequence of the insufficient closure of its curtains. Hence, arguing from the corresponding lesions of the aortic valves, there ought to be two different murmurs, characteristic respectively of the stenosis and the insufficiency of the mitral valve—and so there are in reality. But the most remarkable fact is, that only the physical sound of mitral regurgitation was recognised, until within a comparatively very recent period, in the great majority of cases. As the blood is passing from the auricles into the ventricles at the moment of the dilatation of the heart, it ought to follow that in cases of obstruction of the auriculo-ventricular valves, the murmur should be heard either during the diastole or immediately before the next systole—and the latter circumstance really occurs. But, as a matter of fact, this obstructive murmur was either not heard at all, or it was mistaken for the regurgitant one by nearly all cardiac pathologists. Dr. Hope, while admitting the occasional existence of an obstructive murmur of

the mitral valve, attributes the feebleness of its tone to the weakness of the diastolic current out of the auricle, and he found this murmur absent unless the contraction of the valve was considerable; but he also often found it absent when the contraction was *great*, and he was compelled to rely in most cases on the regurgitant murmur as the evidence alike of obstruction and regurgitation. Dr. Hope considered that, on the whole, this murmur was exceedingly rare, and that those authors, including Laennec, who supposed the contrary, mistook it for the murmur of aortic regurgitation. Dr. Walshe, writing in 1867, while describing the obstructive murmur of the mitral valve, admits that it is frequently absent, and that thus the positive diagnosis of mitral obstruction is far from easy. Even where constriction was found after death Dr. Walshe often observed that the murmur was wanting, and he attributes the fact to the weakness of the auricular systole, and to the smoothness of the constricted orifice. Dr. Walshe, however, anticipated the present views in reference to this murmur by regarding it as rather postdiastolic or presystolic, than as actually diastolic in rhythm.

The difficulty, or almost the impossibility, of distinguishing the obstructive mitral murmur, was generally admitted by most other writers and observers, but Dr. Gairdner, on the contrary, writing more recently, regards such a murmur as being of rather common occurrence, and thinks that it is among the most easily detected of all cardiac murmurs. Dr. Peacock has lately examined this question afresh, in consequence of a careful perusal of Dr. Gairdner's remarks, and in a communication made to this Review in October, 1867, he states that he has considerably modified the views which he previously entertained on this subject, and that he believes the direct mitral murmur to be less uncommon than he formerly supposed. Still, however, Dr. Peacock was then far from agreeing with Dr. Gairdner either as to the frequency of the murmur or the facility of its detection, and out of his very extensive field of observation he adduced, in the communication referred to, only two cases where a presystolic, as well as a systolic murmur of mitral origin, was distinctly heard by him, and where the post-mortem examination confirmed the diagnosis. Admitting, then, that a distinction may be drawn between obstructive and regurgitant mitral murmurs, Dr. Peacock justly observed that this distinction was not merely a point of minute diagnosis, but that it explains some secondary pathological changes affecting the heart and lungs, and perhaps leads to some important practical results in reference to prognosis and treatment. Thus in mitral obstruction the consequences of the disease fall mainly upon the left auricle and the

right side of the heart, while in mitral regurgitation the most marked changes occur in the left ventricle and auricle, those cavities becoming greatly increased in capacity. Dr. Peacock thinks also that the prognosis is less serious in obstructive than in regurgitant mitral disease, because the former is more slowly induced, and the system has time to accommodate itself to the altered circumstances, while the latter is often rapidly and even suddenly brought about.

In the fourth volume of the 'Clinical Lectures and Reports of the London Hospital,' 1867, Dr. Sutton confirms the opinion of Dr. Gairdner, that presystolic bruits are by no means very rare, and he adduces a case attended by himself at the Victoria Park Hospital, in which the existence of the murmur in question was distinctly proved during life, and in which, after death, the walls of the mitral orifice were found indurated with calcareous matter, and the orifice itself admitted the tip of one finger with difficulty. In this case the presystolic murmur was heard most distinctly close under the left nipple, and it was separated from the second sound by an appreciable period of silence, and, moreover, it preceded the pulse and the impulse of the heart against the chest-wall. Dr. Sutton concludes his paper by stating that twelve cases had come under his notice during two years, in which there were presystolic murmurs, and he concludes from his experience that the presystolic murmur is not rare, although it is not so common as presystolic combined with systolic bruit. He admits, however, that presystolic murmurs are difficult of diagnosis.

Dr. Flint, writing in 1870, considers the presystolic murmur, or, as he calls it, *the mitral direct or the mitral diastolic murmur*, less frequent than either of the other three murmurs, viz. the mitral regurgitant, the aortic direct, and the aortic regurgitant. But he confirms the fact of its occurrence, and he agrees with those who disbelieve in its rarity. In his own experience, out of 123 cases of mitral lesion, the mitral direct murmur was noted by him as present in 47. He thinks the quality of the murmur is distinctive, and he describes it as being rough, and as resembling the sound produced by throwing the lips or tongue into vibration with the expired breath. As regards the *time* when the murmur occurs, he agrees with most other writers, and thinks the term *presystolic* is quite applicable. Dr. Flint, however, considers that the mitral regurgitant or systolic murmur is that which is most frequently met with in organic disease of the heart. But he argues, from experience, that although a mitral regurgitant current almost invariably produces a murmur, the converse is not equally true that a mitral systolic murmur uniformly denotes insufficiency or regugitation.

He shows that a murmur at or near the mitral orifice may be due to roughness or calcareous deposit, without the valves being insufficient. This important statement appears to foreshadow the present impression in reference to the nature and the import of mitral murmurs and the rhythmic period at which they are heard.

The ideas now current among physicians who are studying the auscultatory phenomena and nature of cardiac diseases are rather to be gathered from the discussions recently held at our medical societies, from the living cases examined at these meetings, and from the scattered monographs in our medical journals, than from any books specially devoted to the subject. It appears from these sources of information that so far from the presystolic murmur being of rare occurrence, it is one of the most common of the auscultatory phenomena of the diseased heart, and that so far from being difficult of diagnosis, its recognition is exceedingly easy. The fact just alluded to as stated by Dr. Flint, viz. that causes of obstruction, such as roughness or calcareous deposit on the mitral valve, may give rise to what was formerly considered as a systolic murmur is quite established, but then the murmur is not a systolic one at all, but a presystolic one. In fact, our views on this subject are now completely revolutionized, and the loud and distinct murmur so often heard over the mitral region is really the presystolic murmur, and denotes the presence of obstruction, while the systolic murmur, if it exists, is posterior in point of time. The true diagnostic character is to be drawn from the pulse, either at the wrist or at the carotids, which *succeeds* the presystolic murmur, but is synchronous with the systolic one.

Now, as in the case of the aortic valves, an obstructive murmur may be heard alone, or be accompanied by a regurgitant murmur, so the same conditions may be found in the mitral valve, where a regurgitant murmur may follow the obstructive one, or either murmur may exist alone. But the view which has hitherto been held as to the nature of the mitral murmur appears to be no longer tenable, and instead of the regurgitant murmur being almost the only recognisable one, it is the obstructive murmur which is actually the more frequent. The latter is really the presystolic murmur, the very existence of which was almost denied by Hope, and pronounced to be of very rare occurrence by Walshe, while the sound which succeeds the presystolic murmur is, if of natural tone, the normal first sound of the heart, and, if rough or blowing, it is the systolic murmur of regurgitation. The combination of aortic obstruction with mitral regurgitation, hitherto considered as the most common of the morbid conditions of the heart, is no longer to be regarded

as representing the true connection of phenomena, but rather aortic obstruction with mitral obstruction. There may, of course, be mitral obstructive (presystolic) *and* mitral regurgitant murmur, but when only one exists it is, in all probability, the presystolic, but the relative frequency of the occurrence of these murmurs, or of their coexistence, must now be studied anew by the light of recent experience and observation.

Disease of the walls of the heart is very often attributable to morbid affections of the valves, and there is a growing disposition on the part of pathologists to regard hypertrophy and dilatation, whether separate or combined, as being caused almost exclusively by valvular lesions either obstructive or regurgitant. Indeed hypertrophy is now generally considered as a compensatory condition in valvular disease, and, on this view, is not a disease at all; while dilatation, on the contrary, being caused by the overflow of blood into the cavities of a heart with weakened walls, which are incapable of resisting the strain put upon them, is a condition full of danger. Dilatation itself, however, is compensated by hypertrophy, and hence the very common combination of dilatation with hypertrophy need not be regarded with too much apprehension, as it is still a compensatory effort of nature to overcome the difficulties of the circulation, the hypertrophy giving strength to the weakened walls. Nevertheless the diseases of the heart-walls ought not to be too exclusively regarded as dependent on valvular disease, inasmuch as they may arise without antecedent valvular affections, and may, in fact, be the causes and not the effects of valvular lesions. The dilatation of the right ventricle, for instance, may, by the mere stretching and expansion of the walls, give rise to tricuspid regurgitation, the curtains of the valve being separated from each other by the widening of the aperture.

Dr. Quain has very recently called particular attention to the diseases of the muscular walls of the heart in his Lumleian Lectures, delivered at the College of Physicians, and has specified the exact changes which the muscular walls undergo in hypertrophy. He has shown that enlargement of the heart may depend on an increase of its muscular fibres, or of its connective tissue, or of its fat, which last condition is not, however, to be confounded with fatty degeneration. He has also called or rather recalled attention to the fact that hypertrophy may arise independently of valvular disease, as for instance, from athletic sports carried to excess, or even from emotions of the mind, or from the wear and tear caused by the excitement of business. From reference to the statistics of the Registrar-General, Dr. Quain shows that heart-disease, among men has greatly increased while in women it has remained almost stationary. But the question presents itself whether the

facts on which these statistics are founded are in all cases to be relied upon, inasmuch as certificates of death are still given in a very loose and unsatisfactory manner, and "Death from disease of the heart" is in many cases a mere *façon de parler*.

The subject of fatty degeneration of the heart has received abundant elucidation of late years, and the recognition of its true pathological characters is one of the triumphs of modern medicine. Since the publication of Dr. Quain's monograph on the subject in the 'Medico-Chirurgical Transactions,' numerous opportunities have presented themselves of verifying the fact of the presence of minute oil globules developed among, and taking the place of, the muscular element of the heart, and the fatal nature of this insidious affection has been only too fully recognised. But unhappily the diagnosis and the treatment of this disease have not gone hand in hand with its acknowledged fatality and its post-mortem detection. We might almost find a subject for satisfaction in the reflection that, as the treatment is so unsatisfactory, the diagnosis is likewise obscure, for of this disease it may indeed be said *hæret lateri lethalis arundo;* and like the arrow the malady is swift and unexpected in its destructive power. Happy is it then for the sufferer that science is unable to distinguish with accuracy a hidden evil which the curative powers of medicine have hitherto been impotent to remove.

In making these remarks we have no wish to detract from the merit of the writers who have done their best to elucidate the diagnosis and the therapeutics of this affection, and Dr. Flint's chapter on the subject is well worthy of perusal, as he has said all that probably can be said in the present state of our knowledge. But we must agree with Sir Thomas Watson in his doubts as to the possibility of an accurate diagnosis being made during life; and as to the treatment of the disease, that it must, in the very nature of things, be unsatisfactory. If it were possible to detect the early deposition of oil-globules among the muscular elements of the heart, there exist no known means to prevent their extension, and when the muscular fibres have been largely replaced by fat, no method has been devised for restoring the healthy structure, and all that can be said of our incompetence to distinguish such a morbid condition, so far as the patient is concerned, is that "ignorance is bliss." The disease may continue for a long time, perhaps without causing much or even any inconvenience, and is certainly not attended with so much suffering as many affections of a purely nervous nature, and, when death ensues, the stroke falls in most cases quickly and painlessly.

not easy to give a wholly satisfactory summary of the

present condition of cardiac diagnosis and therapeutics in our brief limits, and more especially because that which appears established to-day may be invalidated to-morrow by the progress of investigation and by the accumulation of new facts; and, moreover, even well-established and admitted facts may receive different explanations from equally trustworthy observers. By the application of physical research, the diagnosis of heart disease has reached, in many respects, to a point of excellence which at the commencement of the present century could never have been contemplated by the most enthusiastic votary of medicine. But in addition to the means already employed to investigate the movements, the sounds, and the bulk of the heart, namely, palpation, measurement, inspection, percussion and auscultation, a new method of diagnosis has very lately been introduced by the use of the sphygmograph, by which the pulsations of the arteries and of the heart are made visible to the eye, and their varieties made easy of computation, comparison, and delineation. Hitherto, indeed, the instrument has taken rank mainly as an ingenious piece of mechanism, and those who have introduced it have hardly claimed more for it than that it is an exponent of phenomena, healthy and morbid, the existence of which may be ascertained by other means of inquiry. But placed in the hands of the acute and intelligent investigators of the present day, it promises to do much more than to confirm conclusions which may be drawn from other sources, and appears likely to become a guide in some of the departments of difficult diagnosis in cardiac pathology.

As we write, an attempt which will probably prove successful, is being made by means of this instrument to determine the true nature and the cause of the presystolic bruit to which we have lately referred as the indication of mitral stenosis. By the aid of the cardiographic and sphygmographic tracings it is shown that in cases of mitral stenosis there is a prolonged contraction of the hypertrophied auricle during the ventricular diastole, and hence the presystolic murmur is plausibly considered to be in its nature *auricular systolic*, or in other words to be caused by the propulsion of the blood through a contracted orifice by means of an hypertrophied auricle. Not only, therefore, may the nature of the presystolic murmur be fairly inferred by the use of this instrument, but its occurrence in point of time as compared with the systolic or regurgitant murmur may be accurately ascertained.

In the application of therapeutics to the diseases of the heart, if there is no great triumph to record, we may at least congratulate ourselves that some mischievous or useless measures have been abandoned, and that a rational system of medication,

founded upon correct physiological, as well as practical data, has taken their place. If we have found that the inflammatory diseases of the heart, which are undoubtedly, in very many cases, the causes and the forerunners of the organic affections, cannot be checked or controlled by the heroic plans of treatment which were once regarded as almost infallible means of cure, we have at least ascertained that the diseases themselves are not so fatal as was once supposed; and that their consequences, serious as they are, are not always intolerable to the sufferers, nor incompatible with the average duration of life and the enjoyment of moderate health. The diagnosis of pericarditis and of endocarditis, made almost entirely by means of physical exploration of the præcordial region, no longer calls upon us to drain our patients of their blood, *coup sur coup*, as was recommended by Bouillaud, or to saturate them with mercury, as was advised in our own schools. Experience has abundantly shown that such methods of treatment were useless, and in all probability were injurious; and the most satisfactory evidence of the greater success of modern heart-therapeutics is offered by the fact that acute disease of the heart is rarely fatal in the present day. The peculiar poisonous conditions of the blood —whether in rheumatism or Bright's disease—which give rise in general to pericarditis and endocarditis, cannot be exorcised by bleeding or neutralised by calomel, nor can these agents diminish or control the fibrinous exudations which appear to be of the essence of pericardial adhesions, or of imperfect or contracted valves. But by a milder hygienic discipline, and the more cautious administration of curative means, the disease may be in most cases guided to a favorable termination, if not to absolute cure; and nature herself does more in such cases than was once supposed. Man, after all, is but the minister and interpreter of nature, and in our so-called cures we only co-operate with her *vis medicatrix*, and help her in her beneficent processes; and by the soothing treatment of the inflammatory diseases of the heart, whether by occasional moderate depletion, by dietetic regimen, by the use of sedatives, or by other similar means, we are able to achieve more real success than by the energetic attempts formerly made to drive out the disease by a *coup de main*.

The importance of adherent pericardium as a source of suffering is, in all probability, exaggerated; and the supervention of hypertrophy, which once appeared to demand our strenuous endeavours for its prevention, is now, as we have already remarked, usually admitted to be a beneficent provision, rather to be encouraged than resisted, for compensating the inconvenience caused by diseased valves.

Even confirmed valvular disease has been satisfactorily proved to be not incompatible with longevity, and the frequency of sudden death from this same lesion has certainly been overrated. Independently of the coexistence of hypertrophy which aids in forcing the blood through narrowed orifices, or in repelling the regurgitant stream through imperfectly closed flood-gates, the heart itself, in innumerable instances, accommodates itself to the diminished supply of blood in its cavities, and the system accommodates itself in like manner to the diminution of its nutritive supply. While, also, we are unable by remedial measures to rectify the atheromatous or calcareous degeneration of the valves or of the arterial walls, or to supply the deficiencies caused in the valves by accident or imperfection, we may at least assist nature in rendering these lesions more tolerable to the sufferer by the adoption of suitable dietetic, regiminal, and medicinal prescriptions. The efficacy of certain drugs—although they may possess no magic power to "cleanse the foul bosom of that perilous stuff which weighs upon the heart"—is unquestionable, and among the most powerfully remedial in this respect are iron and digitalis. The true effects of the latter are only beginning to be justly appreciated, and time and experience will probably develope still more important qualities in this herb as a cardiac medicine than it is now supposed to possess; but digitalis and iron, like all other drugs, and every other remedial measure, are only the handmaids of nature, and not her opponents.

IX.—The Pathology of Cancer.[1]

So much has been written upon the pathology of cancer during the past few years, and views in many respects so contradictory have been advocated by the various writers, that it is not an easy matter to prepare for our readers a *résumé* of our present knowledge upon the subject.

The very term cancer is now used in such different senses—one school restricting it to tumours having a definite anatomical structure, whilst another prefers to continue the old fashion of grouping together under this one and the same name all growths

[1] 1. *The Origin of Cancer: considered with reference to the Treatment of the Disease.* By CAMPBELL DE MORGAN, F.R.S., Surgeon to the Middlesex Hospital. London, 1872.

2. *Cancer.* An Article in 'Holmes's System of Surgery,' vol. i. By C. H. MOORE. London, 1870.

3. *Die Krankhaften Geschwülste.* By RUDOLF VIRCHOW. Berlin, 1863.

4. *General Surgical Pathology and Therapeutics.* In Fifty Lectures. By TH. BILLROTH. Translated by CHARLES E. HACKLEY. New York, 1871.

which show decidedly malignant proclivities—that a reader unskilled in the subtle distinctions of minute anatomy must be often not a little confused by the apparently contradictory statements of many modern writers. Without ourselves advocating any special view on the subject, we shall endeavour so to condense and arrange the latest observations and hypotheses which have been published as to show how much real knowledge has been gained by so much work, and what are the facts upon which theories so diverse have been founded.

The first thing that strikes us in this inquiry is the melancholy unanimity with which surgeons still urge the use of the knife as the only form of rational treatment, nearly all admitting the ultimate hopelessness of the remedy, but at the same time agreeing that nothing else is of the least curative value. On this branch of the question, however, it is not our intention to write on the present occasion. The importance of the point is too great to admit of its being usefully considered in a short article on pathology.

But treatment is not the only point still requiring to be worked out by surgeons. The secret of the origin of cancer, and of its malignant dispersion through the body of its victim, has still to be made out. What is really the first tissue change met with as the starting-point of a cancer? What share has the blood in the production of this tissue change? How are we to explain those singular instances in which secondary tumours are met with in parts of the body anatomically most distant from the primary growth? Is the existence of a special dyscrasia necessary for the production of a cancer? All these and many more points have yet to be investigated before we can unreservedly accept any of the hypotheses which have been put forth by pathologists of the nature of cancer.

Meanwhile, these hypotheses may be classed under two headings, namely, the "constitutional" and the "local" origin of cancer; and so much confusion has arisen from the circumstance of certain of the writers who have adopted various modifications of these two theories, attaching a different significance to the term cancer, that it is necessary to refer to the histology of new growths before we can discuss profitably their pathology.

Not many years ago surgeons were content to recognise two main varieties of cancer as distinct from other new growths, and to these varieties they gave the names "scirrhus" or hard, and "encephaloid" or "medullary," or soft cancer. The other rarer forms of "osteoid," "colloid," "villous," "melanoid," &c., were regarded as special modifications of one or other of these two groups, and epithelioma was variously regarded as a

distinct form of cancer, or as a growth only allied to that disease by its occasional malignant qualities. All other tumours were reckoned as benign, and as instances turned up from time to time of these also showing signs of genuine malignancy, they were regarded as of too rare occurrence to cause much misgiving as to the propriety of the classification generally adopted. They were spoken of as malignant enchondroma, malignant fibroma, and so on, and carefully separated from the true cancers.

Now, however, all this is being changed. The great impetus given of late years to microscopic work in the examination of tumours—mainly owing to the labours of our brethren in Germany—has shown that this old classification, with which our seniors were well content, is far too artificial and uncertain for general adoption; and we have offered to us the alternative of using the term cancer strictly in a clinical sense, without regard to structural peculiarities, or of restricting it to tumours possessing a given definite anatomical structure, without regard to the clinical history.

If by cancer we mean merely a growth possessing the clinical features of malignancy, *i. e.* a power of infiltrating indiscriminately the healthy tissues, of recurring in the same place after removal, of infecting the lymphatic glands, and of being attended with the development of similar tumours in remote parts of the body, then we must clearly rank in the same category tumours of the most diverse structure. For the instances of so-called innocent growths following a malignant career are now far too numerous to be reckoned as exceptional, and their number is daily increasing as workers with the microscope multiply, and observations are recorded with greater precision.

If, on the other hand, we restrict the term to growths exhibiting a given minute structure, we find ourselves calmly striking out from the list many of the most virulently malignant tumours. For it is quite clear that we have been used to group together, under the head of "encephaloid cancer," many growths of widely different anatomical conformation.

The new nomenclature with which these recent microscopical observations have been clothed has served still further to bewilder English surgeons, for the old-fashioned word sarcoma, once so indiscriminately applied to any solid swelling, now does duty for many of the most important forms of the old soft cancer; whilst the Greek word carcinoma, which we have been wont to use as a convenient synonym for cancer, and a word less likely to shock our patients, has now, with many pathologists, a distinct significance, and conveys an impression of thoroughly marked anatomical peculiarities in the tumour so styled.

According to these modern views, of which Virchow is the

able exponent (his great work on the 'Pathology of Tumours' being the most valuable of all the many contributions to our knowledge of the subject, and one with which all surgeons should be familiar), the various malignant new growths, which we have been accustomed to call cancer, are subdivisible into many classes, of which the most important are carcinoma or cancer proper, epithelioma, sarcoma, glioma, and lymphoma.

In this series carcinoma is applied to a new growth of fibro-cellular material, in which the cells are of an epithelial type (if not, indeed, of epithelial origin), and are remarkable for their diversity of form, and for the manner in which they are grouped in the alveoli of the network formed of the fibroid substance. Within these loculi or meshes the cells are tightly packed, and it is a point specially insisted on by Virchow that there shall be no visible material separating the cells. This structure is best seen in certain forms of scirrhous breast. In the softer kinds of carcinoma the same elements are present, and with a similar arrangement, but here the fibroid stroma is less conspicuous, and the cell growth in its luxuriance furnishes a richer profusion of varied shapes and sizes.

Epithelioma, consisting of cells clearly derived from the pre-existing epithelial cells of either a cutaneous or mucous surface, and clustered together in little masses or clumps in the midst of a fibroid structure, is obviously nearly allied to carcinoma in its anatomy, and by some pathologists, therefore, it is classed with it as one of its varieties, the structure just described as carcinoma being by them regarded as specially modified by its development from glandular elements, but otherwise of the same general conformation as epithelioma.

The important group of sarcomata includes all the growths which are built up of simple cells bound together by a varying amount of semifluid granular material. Herein are grouped, therefore, the tumours which we have been used to call fibroplastic, recurrent fibroid, myeloid, firm and soft medullary cancer, and the like—growths whose capricious display of malignant properties have always made them a puzzle to surgeons, creating difficulties in this respect which have been in no great degree cleared up by the new nomenclature.

Glioma seems to be merely a form of small-celled sarcoma derived from the connective tissue of nerve, but it is an important variety, because it includes nearly all the soft malignant growths of the eyeball in infancy, which are not melanotic.

Lastly, lymphoma is a growth whose structure closely corresponds to that of the follicular portion of a lymphatic gland; and upon its anatomy much light has been thrown by our own distinguished pathologist, Burdon Sanderson.

These several distinctions, by which it is attempted to classify cancers on an anatomical basis, may or may not be of much clinical importance; but it is clear that, if English surgeons are to work harmoniously with their continental *confrères*, they must be at least familiar with the meaning of the terms; and it is not improbable that, as our acquaintance with these varieties ripens, we may find that no little light is thereby thrown upon a confessedly difficult subject, namely, the relation of the clinical history to the anatomical structure of tumours.

Just at present we have not advanced very far in this direction in England, for English surgeons have not yet fully adopted the classification and nomenclature just described. Of the two English writers, for instance, whose names stand at the head of this article—both of them great authorities on the subject—Mr. Campbell de Morgan, although he adopts as the definition of cancer some such formula as we have given here for carcinoma, yet by limiting his account of the pathology of the disease to tumours having that structure, he clearly separates from other equally malignant growths a tumour which, although generally highly malignant, may yet occasionally run an absolutely innocent course. In other words, whilst guarding himself from participating in the old confusion between the terms cancer and malignant, he yet virtually shares in that confusion, by limiting his observations on the origin of malignant disease to the growth having the special qualities of an "alveolar fibrous stroma, and contained un-uniform cells." In Mr. Moore's article, in Holmes's 'System of Surgery,' also, although an attempt is made to distinguish the cancers from other tumours, the separation is not very successfully accomplished, since the grounds for the distinction are not founded on microscopical structure so much as on clinical characters and coarse features; and in this way much of the old confusion attending the term medullary cancer is perpetuated.

In fact, it seems to be now clearly evident that any separation of cancerous from other tumours must be artificial and, indeed, almost useless. It may be difficult to place any given new growth in its proper anatomical division, but we can trust to no other sign by which to classify tumours; and the most that we can say as yet is, that the clinical features of malignancy are far more apt to be associated with one anatomical group than with another. Further research may show that particular symptoms of malignancy are associated with growths of particular minute structure; but we must extend our observations not a little before we can lay down any reliable rules on this head.

So then, in discussing the pathology of cancer, we must be

content to use the term in its broadest sense, rather than in the limited sense in which Mr. de Morgan employs the word; and we shall find no difficulty in applying the arguments used by Mr. de Morgan for carcinoma, as anatomically defined above, to all the tumours which commonly run a malignant course; nay, we can hardly fail to notice that these arguments gain weight by the modifications required in their application to different morbid structures.

The connection of cachexia with cancer has long been a favorite bone of contention amongst writers. The point seems at first sight to rule so completely the higher question of treatment, that we might expect surgeons in favour of operations to pooh-pooh the suggestion of a cachexia preceding the tumour which is its local manifestation; but we find that the great majority of observers, whether operating surgeons or no, hold firmly to the belief in some form of dyscrasia underlying the local outgrowths. And by this is not meant merely an inherited, or at least in-born, tendency to the development of tumours—just as certain individuals are born with a remarkable predisposition to warts or to catarrhs: such a tendency would, probably, be freely admitted by all pathologists; but by cachexia is usually implied some definite morbid state of the blood, which might even prove fatal before distinct tumours were developed, and the intensity of which commonly augments as the diseased masses which have been produced by it continue to increase. The signs by which the presence of this cachexia is supposed to be made manifest are familiar to us all. The loss of flesh, the peculiar yellow pallor, the failure of strength, and the characteristic *facies*, have all been dwelt upon by writers upon cancer as particularly significant of the malignant taint. It is true that these symptoms are by no means always to be recognised, nor, when present, are they always pathognomonic of one disease. It is refreshing, therefore, to find that Mr. de Morgan quietly dismisses them as of no further evidence than that the disease has reached an advanced stage. He says:

"Nothing can be more erroneous than the belief entertained by those who have not had much experience of the disease that there is a cachectic condition in the early stages of cancer. To use the words of Professor Humphry, 'So much is said and written about cancerous cachexia, that cachexia comes to be regarded as a necessary associate of cancer. Hence physicians and surgeons rely upon it as a means of diagnosis, and conceive that a disease cannot be cancerous because the patient's health is good; whereas, in reality, cancer, especially in early and middle life, fastens itself often, I would say oftenest, upon those who are well nourished and florid, who seem the most healthy and robust, and so give promise of long

life and vigour.' This point is of the utmost practical importance; yet it is generally ignored, or, rather, the opposite opinion is generally entertained."

Of course, Mr. de Morgan must here refer to cachexia in the sense of a special blood-taint, with symptoms of its own, besides its manifestations in the form of tumours. The inveterate tendency to the formation of cancers in various parts of the body is, unhappily, too frequently apparent to permit us to doubt its existence. This is what Billroth terms carcinosis, or the cancerous dyscrasia; and it is this which still makes surgeons speak of cancer as so fatal a disease, for, of course, no medicines are likely to correct this tendency. If, however, it could be shown that this dyscrasia manifests itself most frequently in a mild form, and that its later ravages are mainly due to the destructive results of the local tumours, and their multiplication by dispersion of their elements by various means, then surgeons might take courage, and seek to obliterate the first growth with more energy and speed.

And this does seem to be the view which is daily gaining more adherents in Germany and in England. Microscopists have discovered an almost invariable similarity between primary cancers and their secondary growths, whether these be in lymphatic glands or in distant parts of the body; and this similarity has led them to believe—not that there is a special cachexia present, which is attended with growths of a special histological structure, although this may be held by some—but that the secondary tumours are always the offspring of elements derived from the parent growth, and transmitted bodily therefrom, through the medium of the blood, or by some other available channel.

The possibility of these modes of multiplication of cancers has long been clearly recognised, and Mr. de Morgan has gone very completely into this branch of the subject; but we fancy that more observations, and those more complete and accurate than many now quoted, are required before we can admit to the full Mr. de Morgan's conclusions. The almost natural inference from these statements is that, if one could completely cut out the first cancerous growth, one would thereby completely cure the patient or remove the disease. Well, it may be so, but before we can adopt so important and hopeful a view, we must have explained to us those many cases in which the secondary growth occupies such a position as to almost exclude the possibility of the mechanical transference of germs from the first tumour. Mr. de Morgan, indeed, states that such an occurrence of cancer, as a primary disease in more than one organ, is of extreme rarity:—

"It is a recognised fact that cancer rarely returns, either in an organ corresponding to its original seat, or, indeed, in any organ which is the usual seat of the primary disease. . . . Take, for example, an excision of the female breast. The patient remains well for two years, and the disease then returns. If this be a new development, dependent on constitutional causes, it would be strange, indeed, if we did not often find it taking place in the other breast or in the uterus; still stranger that it should occur in nineteen cases out of twenty in tissues or organs which are rarely the seat of primary cancer. Yet such is the fact. Mr. Sibley, in his careful analysis of 520 cases of cancer, observed in the Middlesex Hospital, did not find one in which a recurrence of the disease took place in the ordinary seats of the disease."

Now, this is just one of the points upon which more accurate observations are needed before any useful conclusions can be drawn. For it has been lately shown by Mr. Henry Arnott, in a paper in the last volume of the 'St. Thomas's Hospital Reports,' that in 109 cases of cancer of the breast recorded by him in the annual tables of the Middlesex Hospital, the other breast was affected in twelve instances, that is, in the proportion of eleven per cent.—as high a proportion as is usually observed in regard of such common seats as the liver and the lungs. Nor is this a novel observation, for the fact of the occasional occurrence of cancer in both breasts was well known to Sir Astley Cooper, who made frequent reference to the point in his lectures.

But it further seems to us that if cancerous tumours arise quite independently of any pre-existing taint of the system, we ought to have met with different results from the many experiments which have been made on the subject of inoculation of cancer. The failures of these efforts to inoculate animals with fresh cancer material are undoubtedly arguments in favour of the necessity for some dyscrasia or peculiar capacity of the tissues for the reception and growth of the implanted germs. But it may be questioned whether these experiments can be so far taken as showing more than that it is extremely difficult to give rise to cancer in certain healthy animals by the inoculation of portions of cancers from human subjects. To make such investigations of any absolute value, we must show a susceptibility in some particular race of animals to a disease corresponding to our cancer, and then inoculate one of these animals from another so afflicted.

The very rare occurrence of cancer of the penis in the husbands of women afflicted with uterine cancer seems to be a more cogent argument, although it may be that there are certain conditions requisite for the life and germination of implanted tumour cells, which are not furnished in such a case.

Dr. Charlton Bastian, in an introductory lecture at University College Hospital last year, entered into the question of the specific nature of cancer and the mode of its dispersion, and his views are quoted at some length by Mr. de Morgan, and fairly answered. Dr. Bastian seems to consider that cancer spreads like erysipelas, by inducing a similar morbid action in adjacent parts, and that "we have no more to do with a kind of implanted something increasing by a multiplicative reproduction in the one case than in the other." He also relies upon the local conditions being favorable to the growth for the due production of the secondary tumour, and in this connection points to the capricious manner in which these secondary growths follow the primary one, so that one can no more venture to fix upon the internal organs which will be affected in cancer than in syphilis.

After all, however, these opinions do not touch the point at issue, namely, whether secondary tumours are the result of a general taint of the system, or whether they are the mechanical consequences of the structure of the primary growth. For, although it is impossible in the present state of our knowledge to deny that a new growth may extend by infecting the cells of the surrounding natural tissues, so as to cause them to assume different properties and powers, rather than by simply increasing by hyperplasia of its own elements, yet the presence of the original germ, which has been implanted or transmitted, has to be accounted for; and this may be in the case of secondary growths, by reason of the constitutional taint, or by the loosening and carrying off of elements from the first local tumour. Nor is the irregularity of the course followed by these secondary tumours in any way accounted for by the assumption of a special aptitude in certain tissues and organs for their development; such irregularity is more plausibly explained by the anatomical differences between the many forms of cancer and the various peculiarities which may attend the distribution of neighbouring blood-vessels and lymphatics.

Still the fact remains that it is often impossible to account for the occurrence of out-of-the-way secondary new growths by any reasonable theory of bodily transference of tumour germs from one place to another, and so long as this difficulty remains unsolved, and so long as experimenters fail to inoculate cancer, those who have been accustomed to regard the disease as a distinctly constitutional affection will not readily adopt in its place anything so simple and hopeful as the theory of a local ailment increasing by dispersion of its germs and their growth in their new situations.

Mr. de Morgan thus sums up the results of his speculations

upon the meaning of the facts bearing upon the question of the nature of cancer, and as the view set forth is one which is daily increasing in importance, we will quote his words at length. He says—

"The general conclusion at which I should arrive is, that in some persons, and in some parts, there is a tendency, local in its origin, to the formation of tumours. That this tendency may in some have been implanted in the tissue, even in its embryonic condition, though the actual development may not take place till years after birth; in others, although there may be a disposition to morbid growth, the actual tumour will not be developed, unless under some irritation. That the morbid growth having once taken place, it will remain localised, or become diffused, in proportion to the facility with which its elements can be taken up and carried off by the structures amongst which it lies. That the period during which these elements may remain dormant is indefinite. That, save in degree, there is no real difference between the malignancy of cancer and that of some other forms of tumour, and that even the line between malignant and non-malignant growth is not clearly defined. I should place cancer, then, at the top of a scale, at the lowest point of which might be placed the simplest forms of outgrowth identical in structure to the affected parts."

It must be observed that the word "local," as employed by Mr. de Morgan and those who hold similar views, merely implies something which only affects a limited area of tissue. A fatty tumour in this sense is strictly local, for its presence, although more or less inconvenient to the neighbouring parts, and so to the patient bearing it, yet exercises no influence upon the general health of the patient, and, if situated tolerably conveniently, may be borne without sensible discomfort during a lifetime. But of course it might be argued that to set that overgrowth of fatty tissue at work, some general constitutional taint, located, possibly, in the blood, as the fluid saturated with all the special properties of the tissues which it feeds, and from which it is in turn derived, must have been previously existing, and that hence the production of the fat swelling. This pre-existing dyscrasia is still more requisite to explain those not infrequent examples of multiple fatty tumours. But this much being granted, it yet remains to be remarked that this dyscrasia is of no further nuisance to the patient than as the means of producing these tumours, and the tumours themselves are comparatively harmless affairs.

The one and essential difference, according to many modern pathologists, between these lipomata or fat tumours and cancers, is, that when a cancer has once begun to grow, it is not content to remain as an encapsuled mass, but the minute anatomical

elements being loosely grouped together are very prone to dispersion to other parts of the body, and so to cause not only an indefinite local extension, the rapidity of which augments largely as the bulk of the tumour increases, but also, too soon, the formation of similar growths in inaccessible regions, where, by interference with important vital functions, they rapidly undermine the health and destroy the patient.

There seems little doubt that to a large extent this theory is true, allowance being made for certain difficulties in the way of its universal application, which we have already mentioned. It follows naturally that since tumours of almost any structure are occasionally malignant, we must not attempt to separate cancers by any hard and fast line from innocent formations, but, having regard to the special anatomical conformation present, and to the peculiarities of the position of the swelling, we must fortell, as well as we can, the probable course of the tumour, and generally regard those growths as most cancerous whose microscopic elements are so arranged as to be most capable of free dispersion.

What seems to be specially wanted in England just now is, something like uniformity in the nomenclature of new growths. Until we agree to this, and diligently examine the minute structure before giving a tumour a vague name, merely suggestive of its malignant properties, we cannot hope to make much progress in the study of the pathology of cancer, and improvements in rational plans of treatment must remain in the distance. The growing practice of the younger pathologists in this country, if we may judge from the work done in the Pathological Society of London, is to restrict the term cancer to tumours having the structure of a scirrhus nodule in the breast, but this we imagine is a mistake. The terms cancer and malignant have been too long regarded as interchangeable to make any anatomical restriction of the term desirable, and particularly a restriction based upon points of structure familiar to comparatively few surgeons. The term carcinoma, which is generally used in this sense abroad, has certainly not much to recommend it, but almost any nomenclature would be better than the present confusion, provided that the names be uniformly adopted, and that they be distinctive always of some definite anatomical structure.

We cannot close this article, without a parting reference to Mr. Campbell de Morgan's book, which we have taken as the text for our remarks. As the work of a surgeon of great experience it must of course command wide and earnest consideration, which it will thoroughly repay; and as an epitome of the views held by the school of pathologists, of which he is one of

the prominent leaders in England, it is simply excellent. The practical lesson which it inculcates, also, as to the necessity for early and vigorous treatment, is of the greatest value in these days; but into this subject we have not entered. The pathological portion of the book is that which will be read with the greatest interest, and we commend this portion at least most heartily to our readers.

X.—Galezowski on Diseases of Eye.[1]

After reading through the greater part of M. Galezowski's work with much care we feel we are justified in stating that it is very well written and forms a valuable addition to the literature of ophthalmic surgery. Much less bulky than that of Wecker, who, by-the-bye, is seldom mentioned but to be differed from, it yet contains a good description of all the ordinary affections of the eye, preceded in most instances by a succinct account of the treatment of the parts, and followed by a judicious selection of the plans of treatment that the author has found to be most serviceable.

It is not practicable to do more in this place than just to pick out here and there a point that may interest our readers, and first we would refer to his treatment of tinea or ophthalmic tarsi, or as it is now more generally termed ciliary blepharitis. He recommends the patient to bathe the eyes with a solution of carbonate of soda containing one drachm to eight ounces of water. He further injects the lachrymal ducts with warm water, paints the outer surface once or twice a week with tincture of iodine and prescribes tonics, as arsenic or iron.

In the more severe form of blepharitis glandularis in which the follicles of the cilia are affected, he recommends in addition to the above means that the hairs be pulled out; he touches extensive excoriations with a solution of nitrate of silver, and applies a little of the ointment of the red or white precipitate, and especially recommends the following:—

> Red precipitate ten parts,
> Crystallized acetate of lead, five parts;
> Fresh lard, five hundred parts;
> Oil of sweet almonds five drops. Mix.

A small portion to be applied occasionally to the borders of the lids. In refractory cases he recommends a visit to the

[1] *Traité des Maladies des Yeux.* Par X. Galezowski, Docteur en Médécine de la Faculté de Paris, &c. Avec 416 figures, intercalées dans le texte. Pp. 895. Paris, 1872.

sulphurous thermal baths of Aix-la-Chapelle, and of Bagnères de Luchon.

In regard to pustular ophthalmia, one of the commonest affections of childhood and early youth, he advises the direct application of calomel, providing the pustules have not burst and do not present an ulcerated surface; he thinks acts partly mechanically and partly as a resolvent. Atropine should be dropped into the eye to relieve irritation, large phlyctenulæ should be scarified; internally, cod liver oil in drachm doses, and tincture of iodine in doses of two drops, may be given with good effect. He adds an important caution that when the latter is given the insufflation of calomel should be suspended, since an excessively corrosive substance, the biniodide of mercury, is apt to be formed. Where there is great irritability indicated by intense photophobia and a scrofulous constitution, we have ourselves found the extract of belladonna in small doses extremely useful.

The subject of gonorrhœal ophthalmia is briefly discussed, and the treatment we think is faulty. M. Galezowski recommends cauterisations with the mitigated nitrate of silver, the application of leeches and calomel to salivation at the outset, afterwards antiphlogistic treatment, frequent injections and cold compresses, and even excision of portions of the conjunctiva. In the cases we see in this country good food, and wine, and beer, and frequent injection of cold water prove almost invariably successful without other treatment.

The sections on trichiasis, distichiasis, entropion, and ectropion are excellent, and are so well illustrated that no difficulty can be experienced in following the written directions.

The subject of cataract, to which so much attention has been directed of late years, is of course treated of at length, and is well treated. He appears to have had very good success with the old flap operation, numbering 14 cures out of 17 cases. His results with the modified linear method are not so clearly given; he says he has 26 cases of complete success in 35 cases, leaving 9 to be accounted for. Of these, he says, one was lost totally by general inflammation, and two by irido-choroiditis. Besides these, four cases suffered from iritis or irido-choroiditis, but vision was recovered, to how great an extent is not stated; of the remaining two nothing is said. For soft and traumatic cataracts he recommends a plan that he calls the combined linear extraction, which consists in breaking up the lens by two or three operations of discission with a needle at intervals of a week, the softened lens substance being removed by a linear extraction, either with or without an iridectomy. He says he has had 33 cases, all attended by complete success. The general

rules of treatment after the operation for cataract are clearly given, but do not differ from those ordinarily followed.

The eighth part is devoted to the ophthalmoscope; a few of the principal forms of this instrument, which has so completely revolutionised ophthalmic surgery, are mentioned, as the simple ones of Coccius, Zehender, Jäger, Gillet de Grandmont, and Galezowski, and the binocular ones of Giraud-Teulon, Laurence, and Heisch; and most of them have illustrations appended clearly showing their mode of employment. M. Galezowski prefers a reflector with a focal distance of 20-25 centimeters, with a central aperture of 4-5 millimeters (the latter we think too large). For the examination of the erect image he places a concave lens, of 8 or 10 inches focus, behind the mirror. The lens for the inverted image should have a focal distance 2½ inches. The general rules enabling the interior of the eye to be exposed by the student are briefly given, but are scarcely sufficient to enable one who has never had the opportunity (now happily a thing of rare occurrence) of seeing it used to apply it for himself. The diseases of the vitreous are considered, and he does not lose the opportunity of having a hit at M. Wecker, who is in favour of the existence of a definite structure and organisation in the vitreous, and consequently admits the possibility of inflammation in it. He himself only thinks that the hyaloid membrane can become inflamed, and considers that most of the other pathological conditions of the vitreous are due to inflammation of the retina or of the choroid, and their results, excluding, of course, cysticeri, foreign bodies, &c. Synchisis or fluidification of the vitreous is stated to be most common in myopic eyes, and a good description of the various forms of mouches volantes is given, but he does not give any rules by which their position in the eye may be known.

Speaking of optic neuritis and optic perineuritis, M. Galezowski recognises two forms. After giving the usual characteristics of the affection, dwelling particularly upon the great development of the capillaries of the disk by which in the early stage of the affection it is accompanied, and which attains its maximum in the case of cerebral tumours situated at the base of the brain, he observes that these capillaries may give rise to red spots resembling hæmorrhages, and suggests as a means of discrimination what appears to be hardly safe practice, namely, to exert firm pressure upon the globe when the capillaries become pale or even disappear, whilst the ecchymoses remain unaltered. The causes of optic neuritis are, he says, first and chiefly affections of the brain, then diseases of the orbit, and finally constitutional affections, as syphilis, albuminuria, and rheumatism. In drawing conclusions, however, attention must always be paid to the

general symptoms, as no positive diagnosis can be drawn from the aspect of the papilla alone. The treatment is unfortunately in general of little service, yet M. Galezowski thinks counter-irritants, leeches and mercurial frictions may in about ten per cent. of all cases lead to recovery.

In regard to atrophy of the optic nerve, he admits the existence of five species: progressive atrophy from cerebral or spinal disease; atrophy from morbid changes in the vessels; atrophy consecutive upon optic neuritis; atrophy consecutive upon congenital or acquired pigmentary retinitis; and lastly, atrophy dependent on intraocular pressure. The symptoms and characters of each of these are given in detail. He does not admit the existence of Mr. Wordsworth's tobacco amaurosis, or at least admits it with the greatest reservation. There are no new suggestions in regard to treatment; perhaps the most likely plan, if the structure of the nerve be not too completely destroyed, is that recently proposed of the subcutaneous injection of strychnia; it is at all events worthy of trial in every case.

In regard to choroiditis, he admits the following varieties:—1. Choroiditis atrophica (disseminata et generalisata), characterised by more or less perfectly circumscribed spots of whitish colour, surrounded by pigment, and often closely resembling exudations. The spots are usually round, and make their appearance near the posterior pole of the eye. 2. Staphyloma posterior. 3. Syphilitic choroiditis. 4. Glaucoma. 5. Sympathetic ophthalmia. 6. Suppurative choroiditis. Besides these, he gives briefly—Rupture, apoplexy, and separation of the choroid; tumours of the choroid, amongst which he includes tubercle, osteoma, and cancer; and lastly, coloboma and albinoism of the choroid.

Two important sections in M. Galezowski's work are devoted to the medico-legal aspect of ophthalmic medicine, and to the hygiene of vision, subjects to which sufficient attention has not yet been given, and which are scarcely more than alluded to in other works. The former section embraces a consideration of the diseases of the eye which exempt a man from military service, simulated diseases, and wounds of the eye from a forensic point of view. We read with some astonishment that, in accordance with *l'instruction ministerielle*, the French regimental surgeons are directed to reject recruits suffering from enlargement of the lachrymal sac and lachrymal fistula, from ecchymosis or simple chemosis of the conjunctiva, or even from pterygium. A large number of able-bodied men must escape the conscription if these claims for exemption are rigidly accepted. All of them, as M. Galezowski remarks, are curable,

and should not be allowed to stand in the way of admission into the military service. It is different, however, with cases of true granular conjunctivitis or trachoma, with symblepharon or anchyloblepharon, ptosis, central ulcers of the cornea, and their consequences; the various forms of choroiditis and cataract, all of which are substantial grounds for exemption. Besides these there are, of course, the affections of the refractive media, and the diseases of the retina and exophthalmia. In France a man is only exempted from service on the score of myopia when he can read with No. 4 or 5 biconcave glasses at the distance of one foot, or when he can see distant objects tolerably well with No. 6 or 7. In regard to hypermetropia, the 'Conseil de Santé' (1862) gives no definite account, but state only in general terms that it must be of high degree. M. Galezowski thinks that all those who cannot read No. 15 of Snellen's text types without the aid of Nos. 5, 6, or 7 convex glasses, ought to be rejected.

The discovery of simulated affections of the eye is by no means so easy as it would at first sight appear to be. An intelligent man who knows what he is about, and has some idea of the tests to which he is to be subjected, will impose upon the surgeon with the greatest facility, unless the latter is perfectly acquainted with his subject. Fortunately, most patients of this kind are ill-educated, and possess a very slight acquaintance with the laws of optics. In order to ascertain the degree of myopia, if stated to be present, various means may be adopted. Atropine may be dropped into the eye, and the accommodation paralysed, though this is not to be recommended on account of the great impairment of vision the wide dilatation of the pupil often produces. Perrin's optometer may be used, but Boisseau's plan, the *epreuve par surprise*, is one of the best when simulation is suspected. A No. 4 or 5 concave glass is placed before the eyes of the patient, and it is seen that he cannot or will not read with them. Then in a confident tone of voice the surgeon says, "Ah, I see what it is," and at once selects a pair of plane glasses, when the pretended myope will sometimes read without hesitation. Instead of this plan, M. Galezowski prefers to try successively the concave glasses Nos. 3, 4, and 5, then to make him read without glasses, and gradually to increase the distance between the book and the eye. If the patient can read further without the glasses than with them it is evident he is an impostor. Amaurosis is, however, more frequently simulated than any other disease. With the improved methods of examination now at our disposal, an attempt at fraud should rarely, if ever, be successful. The fundus should, of course, be carefully examined, and if no disease be found and deceit be suspected, it

may generally be discovered by the judicious use of prismatic glasses. M. Galezowski gives the following case from his own experience, which is interesting from the youth of the culprit:—

A boy of eleven years of age complained of not being able to see with the left eye. His mother stated that he was very industrious, and much distressed at being unable to work any more. M. Galezowski discovered the deception by means of a prismatic glass, and stated the fact to the parents, who were present at the examination. To his astonishment the mother expressed herself indignant at the idea of her son being guilty of such an offence, and he was obliged, to satisfy her, to make her use the glasses herself. The boy at once confessed his fault, and stated that he had only simulated blindness in order to visit Paris.

Another mode of discovery of simulated amaurosis is by means of the optometer of Flés, which consists of a box resembling a stereoscope, but with a pair of mirrors arranged so that they give crossed reflections of two objects fixed closed to the eye-holes. Javal holds a ruler in front of the eyes, when the patient, taken by surprise, will sometimes read words that are only visible to the supposed amaurotic eye.

The section on wounds of the eye, considered from a medico-legal point of view, though short, is interesting. He gives various cases showing the proneness to exaggerate in those who have received an accidental or intentional injury, and the disposition to attribute particular symptoms, that have long been pre-existent, to the effects of a blow. Thus, in one instance, the Sieur T—, *maitre d'étude* in a large establishment, received a blow with a stone upon the eye, thrown by the son of the Prince of M—. A judgment given in the 1*re Chambre du Tribunal*, charged Professor Tardieu and two other surgeons to examine the plaintiff. This examination demonstrated that the defect of vision was caused, not by the blow of the stone thrown by the boy, but by ptosis and deviation of the left eye, which had existed from youth; and in this opinion M. Galezowski, who was subsequently consulted, fully agreed. Defects of vision, though often complained of after railway accidents, appear to be really rare, or, at least, such accidents are rarely accompanied by any physical lesion visible either to the naked eye or with the ophthalmoscope. Unless cataract or actual lesion of the retina, in the form of detachment, hæmorrhage, &c., be present, much caution should be exercised in accepting the statements of a patient, and it must be considered whether the conditions present are not commonly associated with prior disease, as with myopia, and have no immediate relation with the injury.

Though of course mentioned only to ridicule it, we are surprised to see a section devoted to the disproof of the absurd statement that went the round of the American papers, to the effect that the last scene of life was photographed on the retina of the dying man, and which, it appears, was actually the subject of a report by Dr. Virnois.

The chapter on the hygiene of vision includes observations on the management of infants and children, the influence of various professions, and advice to those following different occupations, as those of printing, harvesting, washing, working in the manufacture of steel, lead, tobacco, and sulphuret of carbon (used for certain india-rubber goods), which are sufficiently sensible.

The work is illustrated by an extraordinary number of woodcuts, most of which are very good. Here and there, however, they are indifferent, as, for instance, cut 112, intended to represent abscess of cornea with hypopyon; cut 128, intended to show the mode of abscission of a staphyloma; and the drawings of the retina, which are out of date.

We recommend the book to our readers.

XI.—Surgery of the United States Army.[1]

This report is extremely creditable to the department from which it has issued, as well as to its editor, Assistant-Surgeon Geo. A. Otis. It consists of a summary of the more important cases which have occurred in the practice of the United States army surgeons during the last five years. As compared with its predecessor, the quinquennium which it embraces has been a peaceful one. "The war of the rebellion" was past, and comparative quiet prevailed over the whole central portion of the North American continent. The report before us may, therefore, be regarded as an average one—not including the statistics of any great battle, or of any extraordinary expedition by land or sea, but simply presenting the usual routine of cases with which the medical officers of the United States Army have to deal. Regarded in this light it gives us a vivid picture of the vast extent of the States, of their varied populations, and of the lawless condition in which some portions still remain. It pourtrays a state of things of which we, in this small overcrowded country, find it difficult to form an idea. We have

[1] *A Report of Surgical Cases treated in the Army of the United States, from 1865 to 1871.* War Department, Surgeon-General's Office, Washington, Aug. 17th, 1871.

read reports by the surgeons of the British army serving in our colonies which, for strangeness of detail, match anything that is contained in this volume. But the cases here recorded all occurred within the limits of the States themselves, and within the area which is under the immediate supervision of the home government. These are not encounters with outlying savages, but with tribes and races who are rapidly being absorbed into the American people, and are adding their strength and sinew to the great transatlantic nation. Viewed from this standpoint the report of surgical cases in the army has a national as well as a professional interest, and will amply repay perusal.

To go through the report in detail—to analyse an analysis—would probably not be very interesting to our readers. What we propose to do, therefore, is to take a general view of it, noticing such points as are of the greatest importance in their bearing upon modern surgery, more particularly those wherein American differs from British practice. In this way we hope without wearying our readers to give them a tolerably accurate idea of the contents of a somewhat voluminous official document.

It is satisfactory to notice at the outset that there is a broad general agreement between the practice which prevails in America and in this country. Such an agreement cannot but be received as an indication of something like certainty in the principles upon which we act. We are often reproached with the differences which exist among doctors, and to ourselves it is a matter of regret that the rules of our art vary so much from generation to generation. But the case is not so bad as it seems at first sight; and when we read such a report as this, a report which embraces the records of all manner of surgical diseases, and when we find that the treatment adopted was substantially the same which obtains in this country, as well as in other countries of western Europe, we may venture to hope that some of the fundamental principles of surgery are becoming tolerably fixed and certain. And, indeed, it is just such reports as the one before us which have helped to bring about this desired end. The naval and military services—from the entire control which they are able to exercise over their patients, and from the length of time which they keep them under observation—have peculiar facilities for producing full and accurate reports, and also for drawing up well-classified statistics. And these it is which have done so much of late years to render our deductions solid and trustworthy. We welcome the volume before us not merely as an interesting record of remarkable cases, but as an aid to the general advancement of our art, and as an addition to the class of tabulated and statistical works to which modern surgery is much indebted for its progress.

But this report is not altogether made up of statistical tables. Far from it. They are only the summing-up of cases which are noted at length, and with considerable graphic power. The details of many of these, more particularly of the circumstances in which wounds or injuries were received, are often very striking and vivid, and relieve the report from the character of dulness and dryness which pertains to many such documents. We cannot, for instance, read without sympathy of the young soldier, "of splendid physical organisation," who was in the habit of bathing in the Arkansas river, at a place where the water was usually eight feet deep, but so muddy as to render an object invisible at six inches. One morning on diving he struck his head against the bottom, which was sandy and constantly shifting, and on this occasion was only covered by eighteen inches of water. The unhappy bather broke his neck, and died in a few days.

We are accustomed to think of scalping as an indignity to which trained soldiers are not subject, but the soldiers of the United States army are in danger of running this risk from the Indian savages. And hard by such barbarity as this we have the evidence of advanced civilisation. We read of railway accidents; in one of these a soldier who was travelling on the top of the "cars" saw that a collision was inevitable; he leapt down, and thereby sustained very serious injuries.

The variations of climate to which the men are exposed are also extreme. In some of the States the heat of summer is intense, while in others the cold of winter is almost arctic. Thus, we read of a lieutenant travelling on horseback across Texas who was overtaken by a severe "norther" during which the mercury fell below zero. He had previously got wet, the consequence of his horse falling in a river; in this condition he dismounted, was overcome by the cold, and remained for two days lying in a half delirious state amid the underwood. It is not wonderful that he lost both his feet by frostbite; the marvel is that he escaped with his life.

Again, a private was going on furlough to his home in Minnesota. "After being three days out in the stage the party of which he was a member being overtaken by a severe snow storm, which continued three days, was obliged to leave the stage in a snow drift on the prairie distant about 110 miles from the place of destination. He wandered over the prairie that day and night, and the following four days, through the storm, freezing his limbs, nose, ears, and cheeks, taking no food or water, until found in a dying condition by Indian scouts, and taken to a station house on the road, on Dec. 17, 1865." The result of this severe exposure was that both his arms and his

legs were frostbitten, and had all to be amputated. "On April 28, 1866, the patient was mustered out of service with his company, but remained under treatment until June 19, 1866, when he left for his home, perfectly recovered."

The lawless condition of parts of the territory is shown by the following extract: "Mentieres, a Mexican, aged 32, employed in the quartermaster's department as a teamster, was received into the post-hospital at Fort Union, New Mexico, on May 11, 1867. Early that morning he had had a difficulty with another Mexican, employed in the same train, which resulted in his drawing a pistol on his opponent, who instantly drew his revolver, and fired at Mentieres, the ball taking effect in his hip."

We have another illustration of the same point in the following: "A seaman of the United States navy was wounded in the line of duty while an attack was being made on a piratical vessel in Tecapan river, west coast of Mexico, June 17, 1870. A conoidal musket ball, entering the left nates, midway between the great trochanter and point of the coccyx, fractured and comminuted the head and neck of the femur, and lodged nearly in the centre of its head." In this case excision of the hip-joint was performed with a successful result.

These instances are enough to justify what we have said of the varied conditions in which the United States army performs its duty, the extremes of climate to which it is subject, and of the semi-civilised and savage men with whom it is brought in contact; while side by side with this are evidences of high civilisation and of advanced scientific knowledge.

We shall now proceed to indicate a few of the features of peculiar surgical interest presented by this volume, features to which our experience in this country affords no parallel, and then we shall notice the points of difference and of likeness between American and English practice.

We find a long and interesting chapter devoted to the consideration of wounds inflicted by arrows, a class of injuries of which British surgeons see nothing. The report does not lead us to suppose that the poisonous character of the arrow has much effect; indeed, it seems doubtful whether these missiles are charged with poison at all, or whether, if they are so, the poison produces any appreciable result. The real danger lies in the arrow-wound itself, and in the dexterity with which the Indians inflict "multiple wounds," firing their arrows in rapid succession as soon as they see that they have got the range, and that one has hit the mark. The following case supplies a good example of these multiple arrow-wounds:

"Private George Osborne, of Troop D, 2nd Nebraska Cavalry,

was wounded by arrows in a skirmish with Indians, near Pawnee Reserve, Nebraska, on June 23, 1863. Eight arrows entered at different parts of his body, and were all extracted, except the head of one, which had entered at the outer and lower margin of the right scapular, and had passed upward and inward through the upper lobe of the right lung or trachea. Hæmorrhage at this time was so great that all hope of recovery was abandoned. The patient, however, rallied, but continued to suffer great pain upon swallowing or coughing, and occasionally spit blood. In July, 1866, more than three years subsequent to the receipt of the injury, he called at the office of Dr. J. H. Peabody to undergo an examination, with a view of applying for a pension, stating that his health was much affected from the presence of the arrow-head. He was much emaciated, and expressed himself tired of life. Upon probing through a small fistulous opening, just above the superior end of the sternum, the point of the arrow was found resting against the bone, about an inch and a half below, the head lying flat against the trachea or œsophagus, with the carotid artery, jugular vein, and nerves overlying. After some little difficulty the point of the arrow was raised above the sternum, and it was extracted without the loss of an ounce of blood, the edge grating against the sheath of the innominata artery during the operation. The missile measured an inch at the base, and was four inches long. . . . The patient, appearing highly gratified at the result, rode to his home; and in January, 1869, the operator reported him perfectly well." (P. 145.)

Another peculiar class of cases met with by the American surgeons, and which are almost unknown to us, are the bites inflicted by venomous snakes; but this species of injuries, though caused by rattlesnakes, scorpions, and water-mocassins, does not seem to be very fatal. In some parts of our Indian dominions they are much more formidable, as we may learn from what the editor says on the subject:

"Dr. J. T. Fayrer, of Calcutta, has had the kindness to supply me with his work on the 'Thanatophidia of India,' soon to be published in London, in which the mortality from snake-bites in Bengal and adjacent provinces, including an area 'rather less than half that of the peninsula of Hindostan,' was in the year 1869 not less than 11,416; and it is his belief that the annual mortality in British India from this cause is not less than 20,000. I question whether so many deaths have occurred on this continent from this cause within the historical period."

With regard to differences between English and American practice, we observe that ether still holds its ground on the other side of the Atlantic, and that the favourite anæsthetic seems to be a mixture of ether and chloroform. Here ether has been tried over and over again, and indeed, it is now used to some slight extent, but, as a rule, it does not enter into com-

petition with chloroform. Very recently the value of various anæsthetics formed the subject of a debate at the Royal Medical and Chirurgical Society; and it was evident, from the tenor of the discussion, that ether was not rising in the estimation of the profession.

Nathan Smith's anterior splint seems to be much employed in America, and with good reason. We believe it to be an excellent appliance, and we have ourselves found it most useful. It is, however, very little known in this country, and is scarcely used at all. Perhaps in our palatial hospitals there may be difficulties in rigging it up, which do not exist in the rough-and-ready post-hospitals. But such difficulties are only of feather weight, and ought not to be allowed to turn the scale against the use of a valuable splint.

One of the novelties which strike us as we read the report is Dr. McGill's proposal to leave a periosteal flap to cover the end of the bone after an amputation. This is carrying sub-periosteal surgery a step further than has yet been done elsewhere. To resect a portion of bone and leave its periosteum is an operation with which all are familiar. But to amputate, and provide the sawn end of the bone with a covering of periosteum, is a new suggestion to us. As far as it has been tried, it seems to have been attended with successful results, but the cases so treated are too few to enable Dr. McGill to say whether it offers any advantages over the ordinary method.

Another novelty is the "ice-poultice." This we have no doubt may sometimes be a convenience when a moderate degree of continuous cold is needed, short of that produced by an ice-bag, and when irrigation is impracticable or unsuitable. The way in which the ice-poultice is made is thus described, "Take of linseed meal a sufficient quantity to form a layer from three quarters to an inch thick; spread on a cloth of proper size; upon this, at intervals of an inch or more, place lumps of ice of convenient size—of a big marble—then sprinkle them over lightly with the meal, cover with another cloth, folding in the edges to prevent the escape of the mass, and apply the thick side to the surface or wound. Closely enveloped with the meal, the exclusion of air retards the melting of the ice, and the thick layer, intervening between it and the surface, prevents painful or injurious contact. The linseed meal is better than bran or similar material, because its mucilaginous properties render it somewhat tenacious and adhesive."

The points in which English and American practice coincide are much more numerous than those in which they differ. Not only is this true of the principles which govern the selection of cases for operation—*e. g.* in preferring an excision to an

amputation or *vice versâ*; but it is noticeable also in many minor details. We observe that upon the subject of dressing wounds there is as much variety of opinion and of practice in the United States as among us; and while one surgeon uses "perfectly dry dressings," another prefers wet applications. Again, some follow the old custom and allow wounds to remain open until they are glazed, while others close them up at once, and seal them with Richardson's colloid styptic, or smear them with Lister's antiseptic putty.

Acupressure seems to find very little favour among the surgeons of the United States army, and yet, if it has any real advantage as a hæmostatic over the ligature, one would expect that advantage to be apparent to military surgeons; but the truth is, that it is only in some comparatively slight cases, as in bleeding from the radial, ulnar or temporal arteries, that the pressure of a needle is a readier or better method of arresting hæmorrhage than the ligature: and it is only for such cases that it will hold its ground. But, if acupressure meets with little favour, torsion meets with still less. This method, of which we hear so much at the present time, and in which the surgeons of one metropolitan hospital express great confidence, is not even mentioned in the report. Is this because it is not suited for the exigencies of military practice, though it may answer very well in an institution where dressers and house surgeons are always at hand?

With pyæmia and tetanus our transatlantic brethren seem as little able to grapple successfully as ourselves. Cases are here reported in which a variety of drugs were tried, and a diversity of methods were used, for the cure of lock-jaw, but none of them were followed by more than that moderate degree of success which seems to attend every measure that has been adopted for the alleviation of this formidable disease.

With regard to condurango—the latest specific for cancer—the only mention of it which the report contains amounts to a condemnation, and gives a clue to the way in which its fame was first trumpeted from interested motives and for trade purposes.

Among the many cases reported in this volume some are of great rarity in surgical practice. Thus there is an example of dislocation of the carpus, and an instance of a large polypus growing from the tonsil; and it is scarcely necessary to add that the experience of the United States army surgeons over five years supplies many of these curious wounds and hair-breadth escapes, with which the annals of military surgery abound.

Of the illustrations which this report contains those on stone are well executed and add much to elucidation of the cases which

they accompany; but we cannot say as much for the woodcuts, they are not up to the mark, but form a weak point in a work which is otherwise brought out in a manner worthy of the valuable material which it contains.

XII.—Acton on Diseases of the Reproductive Organs.[1]

MR. ACTON is well known as a writer who has elected to deal with an aspect of sexual subjects which most surgeons, rightly or wrongly, seek rather to avoid than court. His experience is, on this account, somewhat exceptional, and it is necessary to give a caution against implicit acceptance of all the statements and opinions he advances, many of his views still lacking the general assent of the profession. Were the profession as a body to see with Mr. Acton's eyes there would be some fear of its unduly magnifying a branch of medicine which, while it is by no means to be entirely neglected, on the other hand does not call for very special and exclusive study, and can, in fact, be best dealt with, both in the interest of science and of the patient, by common sense conjoined with sound and broadly based medical knowledge. This, no doubt, may be said to apply with equal force to nearly all specialties, but it is peculiarly applicable to the subject in question, which particularly requires to be dealt with in a broad and catholic spirit, for it is a matter of too much importance, touching so nearly as it does the fair name of the medical profession, to bear handling in a careless or disingenuous manner.

Though as we read his book we cannot conceal from ourselves the growing feeling that Mr. Acton at times strains the stringent bonds which professional etiquette and feeling have rightly imposed on all who deal with such subjects as are here treated upon, and appears to write rather for the public than for the profession, it must be conceded that in his writings he has made some useful contributions to our knowledge of sexual subjects. In the present work, however, he appears to deal with his subject from an insecure stand-point, and, conscious of this, to strive, like the illustrious author of 'Lothair,' to be always "on the side of the angels."

We gather from this book that Mr. Acton regards the sexual instinct as about the greatest curse with which humanity is afflicted, or, at any rate, the male portion of it; for strangely enough, and forgetful, it would seem, of the risks women to gratify it will run, he denies that the opposite sex, as a rule,

[1] *The Functions and Disorders of the Reproductive Organs.* By WILLIAM ACTON, M.R.C.S. Fifth edition. London, 1871. Pp. 262.

derive much pleasure from sexual intercourse, and hence that the perils they incur from its indulgence, or the reverse, are really insignificant. There is, perhaps, truth enough in this view to give it a colouring, faint though it be, but we should assuredly err greatly were we to admit its general correctness. So far as Mr. Acton inculcates morality and virtue, we are entirely with him, tinctured though his advocacy be with ostentatiousness; but we may be permitted to express regret that he should so far forget the dignity and truth of physiological science as, by injudicious efforts to make it appear in accord with morality, to imply that it can ever be otherwise—that it can, in fact, ever be opposed to truth and virtue.

Some sensible remarks are found in the part devoted to sexual precocity; they are such as ordinary professional common sense would dictate. Masturbation in childhood opens a serious question. It is the pet subject of writers on functional disorders of the sexual organs. It demands calm and philosophical consideration, and this it rarely gets. The vice is black enough and sufficiently injurious to the organism, but the exaggerated descriptions ordinarily given of it are usually as wide of the truth as they are absurd. We agree with Mr. Acton that it is of the utmost consequence that the habit should be prevented in children, and that great care and watchfulness are required on the part of those having charge of children to exclude all sources of unnatural excitement, and to check manifestations of sexual precocity when they arise. Mr. Acton gives some sound and vigorous advice to those who have charge of boys, and in his conclusions we in the main agree. But he says nothing about masturbation in girls, which is, we are convinced, a great oversight. Girls, as well as boys, masturbate, and the consequences are quite as injurious. Careful supervision and improvement of the general health are the main things indicated in both sexes, and would do more than all the admonitions and floggings can ever accomplish, and though they might involve almost a revolution in the personal hygiene of the young, their influence is much to be desired.

In treating upon the normal functions of youth, as distinguished from childhood, the author, after having just shown that masturbation in the child is fraught with the worst possible consequences, owing to the immaturity of the sexual organs, proceeds with apparent forgetfulness to say that, bad as self-abuse may be in childhood, the sexual temptations of maturer age are infinitely more ruinous if yielded to. He rightly inculcates continence, which he insists, to be laudable, must be voluntary and entire, and he rightly, too, makes a distinction between mental and physical continence. In this part of the

book some inconsistency is apparent. Mr. Acton seems to take pleasure in picturing the horrors of incontinence, and although, on the other hand, he allows that those whom he calls the "semi-continent," feel better after occasional and moderate intercourse, he protests against their continuing this course, which, he says, "perpetuates the disease," that is, the discomfort and unsettled state of mind and body which, in some continent persons in sound health, frequently arise. He adduces as an argument in favour of absolute continence the sufferings of clergymen who, through the illness of their wives, happen to be forced into continence. The fact might be cited with equal force as an argument in the opposite sense by a shrewd opponent, who would probably argue that a man may therefore live so much in fear of the consequences of deprivation of sexual intercourse as to deem it better never to indulge in it. This style of reasoning is obviously unsound. If the plan Mr. Acton implies were possible, it would be about as sensible as preventing a child learning to read for fear he should read rubbish: the risk must be run, and those who have not backbone enough to surmount it deserve to suffer.

Mr. Acton's remarks on aids to continence, however, are in the main excellent and judicious. Celibacy Mr. Acton seems to regard as the proper state of man, who, so he says, always does the best work in this state, utterly ignoring the abundant and overwhelming evidence to the contrary afforded by the great works achieved by married men. "Under 25," says Mr. Acton, "I have no scruple in enjoining perfect continence." We fear that, except by a very small minority, this excellent advice is not likely to be strictly carried out.

We cordially agree in the author's advice against early betrothals and long engagements, which are alike injurious to young men and maidens.

On the evils of incontinence in the young Mr. Acton dilates forcibly. Religion, morality, and self-interest are against it, and the author adduces a variety of arguments, sacred and profane, in condemnation of the practice of fornication. But it is evident that he feels he is wrestling with an enemy strong in natural force, for he will hear of no compromise. His legislation on this subject would, we fear, however worthy its aim, be Utopian.

Masturbation is pictured in colours frightful enough, but sadly lacking the scientific spirit. The professional reader feels in perusing these pages that the author is speaking over his head to a certain section of the public, and not to the medical profession. This fault, indeed, pervades the whole work, which, despite Mr. Acton's disclaimer to the contrary, it is hard to

believe was ever written for the profession. We quite concur in the opinion of an able physiologist whom the author quotes, that half the boy population masturbates, and yet that the resultant consequences are but slight; and we disagree with Mr. Acton, who would seek to magnify these consequences—threatening insanity, phthisis, heart disease, and other frightful evils. And whether bougies should be used in the treatment of masturbation is certainly a doubtful point; in some cases, perhaps, their use may be beneficial, but, on the other hand, there is the chance of suggesting the passage of such a body into the urethra for the purpose of titillation.

The statement quoted from Dr. Ritchie, that cases of insanity arising from masturbation chiefly occur in members of families of strict religious education, is, if generally true, not merely curious, but a scandal to such reputed education.

Strict cleanliness, cold baths, wholesome diet and exercise, are all probably powerful against vicious habits of the kind here referred to, and would probably succeed when measures of a different order fail. It must, indeed, not be forgotten that many of the sexually debased are people of unsound mind: they come of a bad stock, and their sexual vices are but the outcome of diseased nervous centres.

The remarks on "Marriage, including the choice of a wife" —why not of a husband also?—are hardly suited to a medical work, and, like much else in Mr. Acton's book, can never have been intended for the profession. Nor we apprehend is a description of the sexual act, though it be quoted from Carpenter, for assuredly it is both undesirable for the general reader, and unnecessary for the professional. And what good end is served by stating that the female derives less pleasure—and sometimes none —from the sexual act, than the male, even if it were true? Then the descriptions of coitus in frogs, rabbits, deer, &c., are more than undesirable in a non-scientific work. We have already argued that Mr. Acton's work is not written for the profession. Why, else, is such a sentence as the following inserted;—it refers to impotence: "Any symptoms so entirely hopeless are comparatively rare, and I must here again warn my readers against hastily applying to themselves any descriptions or cases which are after all exceptional." Conscious, apparently, of the admission implied in this passage that his writing was intended for the public, he hastens to add a saving clause which the tenor of the whole book negatives, "It must be remembered that this volume is written for the profession, and in the course of its pages I have to describe severe typical cases as distinguishing different phases of the complaints here treated of." Impotence, Mr. Acton remarks, may be temporary, arising from overwork, anxiety and

the like, or permanent, arising from more deep-seated causes. The author's remarks on this subject are generally judicious and sensible. His experience is considerable, and his remarks worthy of attention. He states it to be his belief that trusses may, and do, interfere seriously with the reproductive power in men. We are not aware whether the experience of others is in unison with this; we desire, however, to draw attention to this statement. Altogether we think this the most satisfactory section of the work, and should be glad were we able to speak of the whole as cordially as we can of this part.

We quite concur in the author's remarks on marital excess—an evil little recognised. Involuntary emissions Mr. Acton treats mainly by cauterisation of the urethra. His remarks on this subject are practical, though not exhaustive. Here, as elsewhere, Mr. Acton is valiant against quacks, whose iniquitous practices he justly reprehends. He does not, however, do justice to his own profession, which is certainly less ignorant of his specialty than he would lead his readers to believe.

The author makes some remarks on infecundity, unfruitfulness, and barrenness, owing to disorders affecting the semen. We concur in his opinion that our present knowledge upon this subject is deficient. The author's observations upon "ungratified sexual excitement" are among the best in the book. There is much truth in the statement that sexual excitement stopping short only of actual gratification has a decidedly injurious effect on some organisms. Sexual sufferings in the married, Mr. Acton's experience tells him, are often of a severe kind. Here, again, he is on sure ground, and is therefore effective, especially in his reprehension of strong-minded women who "assert their right" to refuse cohabitation with their husbands.

"Disappointment in love," the author shows, has its serious as well as its comic aspect, in the suffering that it occasions through deprivation of expected legitimate sexual intercourse.

Spermatorrhœa, the author says, is a complaint "which is as peculiar and as certainly to be distinguished by its own symptoms as fever or any other general disease." It may be caused in several ways—hard study, excesses, nervous affections, &c. It is our conviction that the majority of these cases, whether of supposed or real spermatorrhœa, may be quite as satisfactorily treated by ordinary physicians and surgeons as by those who claim "special" knowledge of the complaint. They belong—if any branch of the profession may be said to have a claim upon them, which we do not admit—rather to those who deal with nervous affections than to those who make a specialty of venereal diseases.

One of the least agreeable features of Mr. Acton's work is the

frequent reference to clergymen, for whom he evinces great consideration, which crops up in its worst form in the section devoted to spermatorrhœa, amongst the effects of which he includes "Clergyman's throat!" This is specialism run mad. Mr. Acton admits that "Clergyman's throat," as he calls it, may arise from other causes, but he has no doubt that in the majority of cases it arises from spermatorrhœa! The most rational explanation of dysphonia clericorum with which we are acquainted is that given by Dr. Walshe, who attributes it to the strained and unnatural tone of voice in which clergymen perform Divine Service. This distinguished physician cogently argues that barristers, members of parliament, lecturers, and others, who speak for hours together day after day, are not specially prone to dysphonia, while clergymen, who only speak in public at intervals during the week, are proverbially subject to it. In spermatorrhœa, as in most of the affections Mr. Acton treats upon, the passage of a sound, and cauterisation of the prostatic portion of the urethral canal, are the remedies upon which he chiefly relies. Of the latter especially he has a high opinion. There are some remarks on "False spermatorrhœa," the supposed complaint on which quacks fatten to the injury of their victimised dupes.

Extreme moderation in sexual indulgence is necessary to be observed by the aged, whom Mr. Acton cautions against "spending much out of their small capital," excess being, he says, the great fault of the elderly. We are glad to be able to praise the remarks on the functions and disorders of advanced life, to which the last section of the work is devoted. They are pertinent and sensible.

Bibliographical Record.

Life and Writings of Dr. Ermerins.[1]—We find in 'L'Union Médicale' for January 4th, 1872, a *feuilleton* from the pen of Dr. Daremberg, with the above title, the substance of which we sorrowfully transfer to our own pages, with the addition of a few observations supplied by personal knowledge and recollection. Francis Zachariah Ermerins was born in 1808, of a good family, at Middelburg in Zeeland, where he received his early education. In 1826, he went to Leyden to study medecine, and became acquainted with Professor Pruys van der Hoeven, who first directed his attention to the study of Hippocrates. The disturbances following the French revolution of 1830, took him away from his academical studies, and he was obliged to serve for one year in the Dutch army. Before he had completed his medical curriculum, he was again called away from the university on account of the first outbreak of cholera, and sent to Rotterdam to attend the patients in that place. At length in November, 1832, he took the degree of M.D., on which occasion he printed a remarkable thesis (4to, pp. 169), entitled, 'Specimen Historico-Medicum inaugurale de Hippocratis Doctrina a Prognostice oriunda.' Without entering into a detailed account of this work, it will be sufficient to mention that by an exact comparison of different works in the Hippocratic Collection ('Prognost.,' 'Prorrhet. I.' and 'Coac. Prænot.'), he endeavours to determine the relative order of their composition. The results to which he arrives were at first (1839) admitted by M. Littré ('Hippocr.,' tome i, pp. 244, 351), but afterwards (1853, tome viii, p. 628), owing to Dr. Daremberg's objections, partially rejected by him, as they were still later (1859) by Ermerins himself ('Hippocr.,' tome i, Proleg. p. xvii). But notwithstanding this apparent vacillation (which, indeed, is partly due to the uncertainty that still clings to various points of criticism relating to the Hippocratic Collection), his inaugural thesis was justly considered a very valuable work, and gave promise of others still more important.

He first established himself as a physician in his native town,

[1] *Notice of the Life and Writings of Dr. Ermerins*, Professor of Medicine in the University of Groningen.

where, notwithstanding the time occupied by his patients, who soon became pretty numerous, he continued to pursue his favorite studies on ancient medical literature.

In 1839 he paid a visit to Paris, where he transcribed several short unpublished medical MSS. (which were printed in his 'Anecdota Medica Græca,' Lugd. Bat., 1840, 8vo), and collated the MSS. of Aretæus contained in the (then called) *Royal* Library.

In 1839 he wrote in the 'Allgemeine Hallische Literatur-Zeitung' a critical examination of the first volume of M. Littré's excellent edition of 'Hippocrates,' and the 'Addenda et Corrigenda' prefixed to his second volume, show the value which that editor attached to his observations. He continued his Hippocratic studies, and in 1841 published (Lugd. Bat., 8vo), an edition of the treatise 'De Victus Ratione in Morbis Acutis,' to which were added critical observations on the work of Soranus, then recently printed for the first time after Dietz's death, and of which he himself long afterwards published a new edition (Traj. ad Rhen. 1869, 8vo).

In 1844 he was appointed Professor at the University of Groningen, where he resided till his death. At first, as is usual in some universities, he lectured on different branches of medical science, but after 1851 he confined his lecturing to general pathology, pathological anatomy, and histology. He, however, continued his private practice, and was also professor of clinical medicine at the hospital of the university. His inaugural oration was entitled 'De veterum medicorum interpretis munere a medicis non recusando.'

In 1847 appeared his edition of 'Aretæus,' which was at one time intended to have been published under the auspices of the (old) Sydenham Society. However, he was not disposed so far to modify the plan of his proposed edition as to suit the requisitions of the Council of the Society, and the result has been advantageous to the literary world, inasmuch as there are now *two* good modern editions of 'Aretæus,' instead of *one*.

After twelve years' delay (caused partly, no doubt, by the slow appearance of the latter volumes of M. Littré's edition), he published the first volume of his 'Hippocrates,' which he finished in 1864, Traj. ad Rhen., 4to, 3 vols.

This is not the place for giving a detailed review of these truly magnificent editions, but of both it may be said (and especially of the 'Hippocrates,' which has no index of any kind), that they are never likely to be used so much as their intrinsic excellence deserves, on account of the expensive and inconvenient form and manner in which they have been printed and edited. With respect to the qualifications of the editor for the task he had undertaken, it may be suggested that he is somewhat too bold in his emendations of the text, and that his notes are too exclusively critical and philological, presenting in this respect a marked contrast to his great con-

tinental rivals (if they may be so called), MM. Littré and Daremberg. It may be added that Galen also was much studied by Ermerins, and with a view to a critical edition of his works. When many years ago the writer of the present notice had in contemplation the preparation of such an edition, he applied for assistance, among other medical scholars, to Ermerins, who entered cordially into the design, and undertook to edit Galen's 'Commentaries on Hippocrates.' It need hardly be mentioned that this undertaking has not been carried out, but part of what Ermerins had prepared for it was published in the second volume of Henschel's 'Janus' (Breslau, 1847), with the title "Emendationen zum Galenischen Text des Hippocrates und Galen's Commentar zum Prognosticon." The writer of this notice never had the pleasure of becoming personally acquainted with Ermerins, but his epistolary intercourse (though practically discontinued for some years), was always both agreeable and instructive. The following sentences, which appeared in a Dutch journal shortly after his death, are translated from Dr. Daremberg's *feuilleton* :—

"While professor at Groningen, Ermerins devoted himself with indefatigable zeal to the interests of science and of the university, employing for work the time even of his meals and recreations. Whatever he once began he finished promptly, without sparing time or pecuniary sacrifices, as is proved by his editions of 'Aretæus' and 'Soranus,' and especially his grand edition of 'Hippocrates.' After having employed himself in clearing up the writings of the ancient physicians, he turned his attention to quite a different part of literature, and began to read the works of the greatest writers in ancient or modern times in German, English, Italian [French], Greek, and Latin.

"To all these eminent qualities Ermerins added great simplicity. He was an enemy of all pretention, and was never heard to deliver an opinion on subjects which he had not himself thoroughly examined. The most striking points of his character were his classical learning, his probity, and his great simplicity, which caused him to be loved and esteemed by all who knew him."

He was carried off at the age of 63, by a short and severe attack of typhus, caught in the hospital, May 29th, 1871, and as having been one of the most eminent medical scholars of the present (or, indeed, of any former) century, it is believed that his name (though less familiar in this country than in France and Germany), deserves this imperfect tribute to his memory.

Claude Bernard's Lectures on Experimental Pathology.[1]— From the preface to this book we learn, with sincere regret, that its

[1] *Leçons de Pathologie Expérimentale.* Par M. CLAUDE BERNARD, Membre de l'Institut de France, et de l'Académie de Médecine, Professeur de Médecine au College de France, Professor de Physiologie Générale au Muséum d'histoire naturelle. Paris, 1872.

author has been six years ill, and that its publication dates from the period of his return to his laboratory, and the resumption of his long suspended labours. But this must not prevent our expressing our opinion as to the work itself, and cautioning English physiologists and pathologists from procuring the volume, under the expectation of finding in it anything new. It consists of two parts. First come twenty-one lectures, which were delivered at the College of France in 1859-60. These are the well-known lectures that appeared eleven years ago in the pages of the 'Medical Times and Gazette.' They were then translated into English by Professor Benjamin Ball, and he has now retranslated them into the language in which they were originally delivered, for the benefit of French readers. In their present form they fill 317 pages, out of the 583 which make up the book. There are besides fifteen lectures, republished from French journals, chiefly from the 'Clinique Européan,' and the 'Révue des Cours Scientifiques.' Only four of them are on special subjects, and these are even more ancient than those contained in the first part of the volume, having been delivered and published in the spring of 1859. They are on fever, diabetes, and the sensitive and vaso-motor functions of the sympathetic nerve. As a sample of their value, we may mention that the only works referred to in the lecture on diabetes, are the very interesting memoirs of Rollo, Nicolas, and Guedeville (1804).

Bernard's own views are summed up thus:—

"In diabetic patients the liver secretes too much. The matter which changes to sugar there cannot be transformed into a product of a more complex organisation. Disassimilation has come to preponderate. Diabetes may, therefore, be considered as a nervous disease, caused by excessive action of the disassimilating nerve of the liver, ... The treatment of diabetes should be directed to the nervous system. If we could galvanize the sympathetic trunk, it would probably be beneficial."

The work which has been done in the last thirteen years by other physiologists in reference to diabetes is thus simply and entirely ignored. The remaining lectures are merely those which formed the introduction to Professor Bernard's course in successive years. In them we have the same ideas repeated over and over again in the most wearisome permutations and commutations. Is medicine a science? Is it a science of observation, or one of experiment? Is a knowledge of absolute truth attainable? What is meant by the cause of a phenomenon?

The reader can imagine for himself the answers that these questions receive. For ourselves we can only say that we have seldom met with so outrageous an instance of bookmaking as this volume represents. The high reputation which M. Claude Bernard has attained by his original researches in past years cannot, indeed, be

destroyed; but the publication of such a book at the recommencement of his scientific labours gives us but little encouragement to expect further progress.

M'Donnell's Lectures on Surgery.[1]—This contribution to surgery consists of five lectures upon syphilis. In the knowledge and treatment of this too common disease, Dr. M'Donnell considers that we do really make progress, as in politics, by the action and reaction of Conservatives and Reformers. As a proof of its importance he adduces the attention to and study of it by some of the greatest minds the world has ever produced; and of its difficulties and obscurities, the slowness of the march of improvement in practice.

Among those who have achieved much for mankind in this respect by giving more power and discretion to "the physician armed with a club," he points with pardonable national vanity to Collis, Carmichael, and Wallace.

He considers that the first great step towards proving that syphilis is a true toxæmic disease, and, therefore, one capable of self-cure, was made by Carmichael, and confirmed by the researches and experiments of Rose of the Coldstream Guards.

The author adopts the modern doctrine, as established by the labours of Lee, Lancereaux, Diday, Puche, and Fournier, and more especially by the report of the committee on venereal diseases, that venereal sores may be syphilitic or simple, the latter form being much more frequent; that the true syphilitic disease may commence in three forms, viz., a dry papule, a chancrous erosion, and a hard chancre, the last being the most common, and the first the rarest; that hardness of the base of the sore, though usual, is not always present, and that, consequently, it is not safe to guarantee the patient from the occurrence of secondary symptems even after the presence of a simple venereal sore. He believes the true syphilitic sore always to have a more or less prolonged period of incubation, as in other toxæmic diseases, from the time of contact to the appearance of the primary symptoms, while the simple sore appears within three or four days. He has been enabled to observe five cases in which this period was ascertained with the necessary scientific precision to be from twenty-seven to thirty-four days, corresponding closely with cases observed by Chausit and Lancereaux (viz. twenty-eight to thirty-five days); and with the results of inoculation as observed by Wallace, Vidal, Necker, and others (viz. a mean of twenty-seven days).

The true syphilitic sore cannot be inoculated on the bearer. This he considers to be proved with a few rare exceptions by the experiments of Ricord, Clerc, Henry Lee, Puche, Poissin, Nadaw, Rollet,

[1] *Lectures and Essays on the Science and Practice of Surgery.* By Robert M'Donnell, M.D., F.R.S., F.R.C.S.I., M.R.I.A., Surgeon to Steeven's Hospital, Dublin. Part I. 1871.

and Laroyenne. The simple sore, on the contrary, as is well known, is capable of inoculation again and again.

The syphilitic sore cannot be cut short by excision or cauterization, however early, of the sore and its neighbouring structures. This is proved by the testimony before the Venereal Committee.

The author considers that syphilis is a true toxæmic poison capable of being entirely eliminated, and not constituting when the system is invaded, a real diathesis, but followed sometimes by a cachexia which is the result often of debility, debauchery, poverty, and mercury combined.

The author does not discard the use of mercury entirely; on the contrary, he considers that in the true syphilis we have no remedy equal to it, but he recommends it to be used as rarely and as carefully as possible. Out of the carefully watched cases of primary syphilis under his own care and very favorable circumstances in the Mountjoy Convict Prison, only two required mercurials to accomplish a satisfactory cure.

The author believes that we cannot by any kind of treatment prevent the constitutional infection, in this respect adopting the opinion embodied in the report, and founded upon the evidence brought before the Venereal Committee. The cases which our forefathers took as proofs of the efficacy of the administration of mercury in the prevention of secondaries were, he thinks, cases of simple ulcer mistaken for true syphilis. He ascribes the honour of the first introduction of iodide of potassium as a remedy to Wallace, and the first scientific study of the natural history of syphilis to Carmichael. With respect to the practice of syphilisation, first tried upon animals by Auzias-Turenne in 1850, and afterwards followed out by Böeck in the human subject, he adopts the opinion expressed (after experiment in 259 cases upon Böeck's plan), by Messrs. Lane and Gascoyne, of London, viz., that syphilisation is not a treatment which can be recommended for adoption," and its results "not sufficient to compensate for its tediousness, its painfulness, and the life-long marking which it entails upon the patient." He agrees with Mr. Gascoyne that an early and uncomplicated syphilis has a great natural tendency to recover of itself, and that this is sufficient to explain the good results which sometimes occur after syphilisation. Cleanliness and the early use of washes and detergent lotions he considers to be a great preventative of the disease. The author thinks highly of warm and vapour baths, either simple or medicated, and of the sulphur waters of Harrogate and Kreuznach, in syphilitic affection of the skin.

The style of the lectures is free, flowing, and easy, and constitutes very pleasant reading, satisfactorily conveyed in a very plain and distinct type. In the last lecture he sums up the testimony of living authorities as to the cases in which mercury should be employed, by

quoting them in the way of question and answer as examined before the Venereal Committee; and his conclusion is that "the vast majority of well-informed practitioners in the present day do not give mercury until they are certain that the case is one of true constitutional syphilis." Most of the eminent hospital surgeons both in England and Ireland, however, still adhere to a moderate course of mercury in the primary symptoms of hard chancre. One, Mr. Erichsen, still advocates its use in both soft and hard sores; while the military surgeons mainly discard its administration altogether, and Mr. Longmore thinks in addition that the secondary symptoms are more manageable when it has not been used. The author concludes his pleasant and instructive lectures by a light and humorous sketch "more Hibernico," of the modern revolution in the domain of King Mercury by which he has lost much of his temporal power, the revolt headed by the Garibaldi of venereal revolutions, the illustrious Ricord, and much forwarded by the "mob orator" Paul Diday.

Gamgee on Fractures.[1]—This, though a new work, is the expansion of various papers and teachings of the author during the last twenty years, especially in relation to the employment of immovable apparatus in the treatment of fractures, a subject to which Mr. Gamgee successfully turned his attention early in life. The lectures are singularly controversial for the audience of students before which they were delivered, but the students of Queen's College, Birmingham, have no cause to complain of want of clearness or of absence of dogmatic teaching on the part of the lecturer, though his views may not in all respects commend themselves to his brother practitioners.

In his first lecture Mr. Gamgee quotes Gross on the importance of the subject of fractures and their treatment (which nobody would deny), and then proceeds to dissect South (on Chelius), Le Gros Clark, and Hornidge for their opposition to immediate reduction and circular compression in cases of fracture in which swelling has already supervened. Seven cases are then quoted to illustrate the success of Mr. Gamgee's treatment by immediate reduction and the use of the starch bandage in examples of fracture complicated with spasm, blebs, extravasation of blood, and inflammatory swelling.

The second lecture is devoted principally to exposing the error of Pott (for whom, however, Mr. Gamgee expresses great admiration) in attributing the displacement, and especially the eversion, of a fractured limb entirely to muscular action. The now exploded teaching of Duhamel and Dupuytren as to the necessity for provisional callus is also severely reprobated since "the treatment of a

[1] *On the Treatment of Fractures of the Limbs.* By SAMPSON GAMGEE, Fellow of the Royal Society of Edinburgh; Surgeon to the Queen's Hospital, Birmingham. London, 1871. Pp. 296.

fracture is perfect in proportion as the thickening at the seat of injury is slight, *as in our cases.*"

In his third lecture Mr. Gamgee discusses the treatment of fractures by suspension and flexion, advocated especially by "the late Matthias Mayor, the impulsive and too exclusive, but always philosophically suggestive surgeon of Lausanne," as contrasted with the method by extension and immobilisation; and the author concedes that the swing is most useful in the treatment of fractures by securing immobility of the fragments, and, though this he would probably not concede, by relaxing muscles which tend to produce displacement. Liston's teaching is approved by Mr. Gamgee, though his practice in respect of the Macintyre splint is reprobated, and Liston's "long splint" is defended from the assaults of Mr. Syme, which are freely criticised. The latter surgeon's method of treating fractured thigh with a splint and folded sheet is shown to be a less complete method of securing immobilisation than the author's favourite starch bandage.

The practice of circular compression in fracture is considered in the fourth lecture, where it is shown that modern authorities who disapprove of it "but echo the teaching of the Macaulay of surgical literature, Mr. John Bell, from whose full and eloquent, always inspiring, though not unfrequently misleading, work" the author gives a long quotation. Mr. Gamgee maintains, on the contrary, that "the gentle and perfectly uniform, the evenly distributed and in no way *constricting* action, which I understand by compression, as applied in the treatment of fractures, is a therapeutic agency than which I know none more demonstrably beneficial," and he quotes several very well-marked cases in support of his views.

In the fifth lecture Mr. Gamgee discusses the cause, physiology, and mode of overcoming spasm in cases of fracture, and reiterates his statements respecting the effects of compression.

The sixth lecture is devoted to the special advantages of compression and immobilisation in fractures complicated with swelling or with penetrating wounds, and has a valuable addendum in a translation of a pamphlet by Professor Vanzetti on the treatment of inflammation by digital compression.

The seventh lecture is devoted principally to the treatment of compound fractures, in which Mr. Gamgee advocates infrequent dressings, and calls attention to Greenway's and Restall's swings for supporting broken limbs.

The eighth lecture is chiefly historical, being a *résumé* of the various views on the treatment of fractures from the time of the Greeks to the present day. A passing reference is made to Mr. Lister's views on antiseptic treatment, only to condemn it as neither new nor true. This lecture is followed by a section of some fifty pages, perhaps the most valuable in the book, giving plain and prac-

tical directions for the construction of the various forms of starch apparatus for the treatment of the several fractures. The writer, by the way, very properly points out that the term "starch bandage" is misleading, since without the addition of paste-board splints the bandage is of little service. He describes also the methods of using plaster of Paris and gutta percha, but his predilection is evidently for the starch bandage with paste-board splints applied over cotton wool.

The work is essentially original in its mode of discussing the subject it embraces, if the method principally inculcated is not entirely novel in the present day. The lectures contain many very useful hints and suggestions, and the author's practice appears to be founded on true principles. At the same time we rather pity the class to whom the lectures were addressed; the self-assertion, the verbiage, the repetition must have all been very trying to an audience, unless carried away by the author's enthusiasm, which may possibly be contagious. As a book the lectures sadly need compression, and the teachings of the author suffer from the form in which they are presented. At the same time we must repeat that the work is one of much value.

Milton on Gonorrhœa.[1]—To write upon gonorrhœa is no easy task. It is a disease which so frequently touches moral questions of great importance to society that it needs to be handled with peculiar delicacy, and it has been treated with such a multitude of "specifics," and in such a variety of ways, that it requires careful discrimination to separate the chaff from the wheat.

Gonorrhœa is a subject to which Mr. Milton has long given a large share of his attention, and indeed the volume before us is only a digest of what he has previously published upon this and kindred topics. It is evident also that he has had extensive opportunities of treating the disease both in public and private practice. He is therefore fully entitled to speak with authority; and yet we doubt very much whether the views he holds and the principles of treatment he advocates will recommend themselves to the present generation of surgeons. In our opinion his remedies are unnecessarily severe for such a simple complaint as gonorrhœa generally is. If we can in a few days cure, almost with absolute certainty, the ophthalmia neonatorum by means of a simple alum lotion used every half hour, why should we have recourse to the much more hazardous solutions of nitrate of silver for the arrest of gonorrhœa? When perfectly safe means are sufficient for our purpose, the surgeon is not justified in using lotions, which may induce stricture of the urethra, though our author makes very light of the risk. Moreover, Mr. Milton is, in

[1] *On the Pathology and Treatment of Gonorrhœa.* By J. L. MILTON, Surgeon to St. John's Hospital for Diseases of the Skin. London, 1871. Pp. 219.

our opinion, much too fond of applying the solid lunar caustic to the interior of the urethra. We thought this practice had well nigh ceased with the death of Mr. Wade.

There is a good deal of repetition in Mr. Milton's work. It might have been considerably curtailed with advantage; and it contains many short digressions, which are irrelevant to the subject in hand. The manner too in which he sometimes turns aside to make an attack upon modern methods of practice, or modern lines of study, is unworthy of a man who has much valuable experience to give and many sound observations to offer. His opinions with regard to gonorrhœa would be more likely to command attention if he did not prejudice the reader by the way in which he puts them forward.

Bradley on Syphilis.[1]—This little volume consists of general remarks upon the origin, varieties, and treatment of syphilis, with an appendix, which contains reprints of papers contributed to the journals, and which have all a connection with the main subject of the work.

Mr. Bradley has evidently had extensive opportunities of studying syphilis, and he is well acquainted with the literature of the subject. But he has not been contented merely to follow others. He has instituted a series of original experiments upon monkeys, kittens, and guinea pigs, and the results obtained by them form the most valuable part of his monograph. These experiments, which are well worth the attention of all who are interested in the questions at issue, go to prove the unity of the syphilitic virus. That there are two well-marked varieties of venereal sores, and that these two varieties generally "breed true," our author is willing enough to admit. But do they owe their origin to one and the same poison variously modified, or are they sprung from distinct seeds? This is the question upon which syphilographers are divided, and in this controversy Mr. Bradley sides with those who hold that there is no sufficient evidence in favour of a duality of poisons. The experiments which he made on animals, and in which he produced soft and auto-inocculable sores by the secretion taken from well-marked Hunterian chancres in the human subject, go to show that the two varieties are interchangeable, and do not always maintain the distinctions, which they certainly ought to present, if they sprang from distinct sources. This is the conviction which has forced itself on the minds of many of our most thoughtful surgeons. While there can be no doubt about the general differences between hard and soft sores, there are so many cases which violate the rule, and which are called

[1] *Notes on Syphilis, with an Appendix on the Unity of the Syphilitic Poison.* By S. Messenger Bradley, F.R.C.S., Lecturer on Comparative Anatomy, Royal School of Medicine and Surgery, Manchester. London, 1872. Pp. 48.

"mixed cases," that we are forced to look for some other explanation rather than to believe that two things which are so often convertible have originated from distinct seeds.

But it is not merely upon the question of unity and duality that Mr. Bradley's little work offers some useful information. The appendix contains a short paper upon syphilitic renal dropsy, and another upon syphilitic post partum hæmorrhage, which are very suggestive. It is only of late years that we have learnt how numerous are the manifestations of syphilis, both congenital and acquired, especially in the internal parts of the body. There are few branches of pathology which have made greater progress than this, and syphilitic lesions are now detected in many situations where the surgeons of the last generation never suspected them. Though Mr. Bradley's observations are too few to warrant him in speaking decidedly, it seems reasonable enough to suppose that in the cases he narrates syphilis was the root of the mischief.

We have lately had to notice in the pages of this Review some large and comprehensive treatises on syphilis. With these Mr. Bradley's little monograph can hardly be said to enter into competition; but, as far as it goes, it is a useful and suggestive book, and indicates original thought and honest work. Though it contains some speculations with regard to the origin of syphilis, and some rules for treatment, to which we should be inclined to take exception, we can in the main recommend it to those surgeons who are interested in the subject—and which of us is not?—as a book which will be read with pleasure, and which can scarcely fail to awaken thought upon some most important propositions.

On Case-taking.[1]—These two pamphlets serve to show the importance which is now attached to case-taking; in fact, it is becoming a thing essential to the progress of our art to have faithful and intelligent reports of the cases which pass through our hands. If medicine and surgery are ever to become more exact and scientific than they now are, it can only be by inductions based upon a large area of well-noted and well-classified cases.. A conviction that this is so is rapidly gaining ground, and in all directions efforts are being made to render more available the vast quantities of material which are annually collected in the case-books of hospitals and infirmaries. At some of the metropolitan medical schools there is an instructor appointed to teach the students how to take notes of cases, and at most of them there are registrars whose duty it is to

[1] *Note-book for Cases of Ovarian and other Abdominal Tumours.* By Mr. SPENCER WELLS. Third edition. London, 1871. Pp. 25.

2. *Suggestions for a ready Method of Recording Surgical Cases in Hospital Practice.* By EDWARD LUND, F.R.C.S., Lecturer on Anatomy, and one of the Surgeons to the Manchester Royal Infirmary. Manchester, 1872. Pp. 17.

keep such records, and to draw up such tables, as shall help to make the work of the wards useful in the advancement of the healing art. But the truth is that to take a case well is a very difficult matter, and presupposes both knowledge and experience together with some facility in writing and expressing ideas. It is, in fact, the applications of the art of the essayist to each individual case; and every schoolboy knows how difficult it is to write a good essay, to touch upon all the salient points of the theme, and to make the whole complete and self-explanatory. Now, just as at the public schools a lad who has the gift of writing a good essay is rare, so is it among medical students. The major part cannot be expected to draw up satisfactory records of the cases that they have under their charge without direction. We have no doubt that when all has been done that can be done by well-devised note-books, it will still require some preliminary instruction and constant vigilance on the part of the surgeons and physicians themselves if their cases are to be well recorded and well indexed.

Premising, then, that we must not expect too much from medical students in an art which is confessedly far from an easy one, and that constant supervision on the part of their teachers is necessary, we turn to the pamphlets before us. Mr. Wells' brochure has now reached a third edition, and is highly appreciated by all who are interested in the particular class of cases to which it refers. As we run our eyes over the pages, and observe how carefully every item in the patient's history which can have any bearing upon the disease is noted and delineated in diagrams, we have a clue to the remarkable success which has attended ovariotomy of late years, and has served to establish its position among the most brilliant of the great achievements in operative surgery.

The main object of Mr. Lund's pamphlet is to recommend a system of abbreviations which have been suggested to his mind by studying those which Donders has introduced into ophthalmic practice. But there is this difference, that, whereas the Utrecht Professor has proposed a limited number of symbols for words which are constantly occurring in eye surgery, Mr. Lund would like to see a system of "long shorthand" adopted, whereby all words and sentences would be abbreviated. To us this method appears difficult, and on the whole liable to give rise to mistakes. To report cases on Mr. Lund's model the student would need to acquire a kind of stenography in addition to all the other branches of knowledge which now claim his attention. Though we heartily concur with Mr. Lund in his desire to see better and more available records of the cases which pass through our great medical institutions, we believe that the unabbreviated system proposed by Mr. Wells is more likely to be generally adopted than the shorter but more complicated one which he recommends.

Lawson on Sciatica, Lumbago, and Brachialgia.[1]—Some books derive a special value from the strictly personal experience they contain. Doctors are often, very often, bad patients, for in the study of their own cases they are apt to lose all mental perspective, and mix up in worrying confusion the grave and trifling symptoms of their ailments. Now and then, however, and especially in chronic disease, their powers of observation assume their best form, and then medical men become capable of educing from their own suffering knowledge of permanent value to their fellow-men. In sciatica, a disease in which the patient is Prometheus-like reduced to a long contemplation of the evil which torments him, we have a condition which the medical mind can turn to profit. This has been well done in the book before us. The author, as we know, was for a long time a very martyr to the disorder on which he now writes, and in these pages he has given us a most graphic description of his own case, and the method by which he was restored to health and usefulness.

The book is divided into ten parts or chapters, of which seven are devoted to sciatica and its treatment, the eighth and ninth to lumbago and brachialgia, while the tenth consists of a short review of the literature of these subjects. As regards the pathology of nervous pains, our author expresses himself strongly against the "visionary hypothesis" of the central origin of neuralgia. He very justly regards this view as purely speculative, and points out very clearly the absence of facts to support it. Venturing himself for a moment into speculation as to the part of the nerve primarily attacked, he says: "I have a strong suspicion that changes of nerve structure commence in those delicate filaments which form such exquisite reticulations on the surface of the sarcolemma of the muscle." His chief ground for this hypothesis is the beneficial action of local injections in sciatica. We can ourselves confirm this observation, and have no hesitation in saying that in some cases the distant hypodermic injection is valueless as compared with injection at the seat of pain. In the remarks on the associated conditions and causes of sciatica there is useful matter, but we regret that the author has not given more space to the connection between dyspepsia and sciatic pain. There are many cases in which dyspepsia and oxaluria form the key to successful treatment, and the frequent association of these conditions with neuralgic pain deserved more consideration. The diagnosis of sciatica, which at first sight appears an easy matter, is, as Dr. Lawson shows by reference to the many different opinions of his own case entertained by high authorities,

[1] *Sciatica, Lumbago, and Brachialgia; their Nature and Treatment, and their immediate Relief and rapid Cure by Hypodermic Injection of Morphia.* By HENRY LAWSON, M.D., Assistant Physician to St. Mary's Hospital, &c. London, 1872.

sometimes a question of extreme difficulty. The word-picture of the affection drawn in these pages by one who is a master of literary art will suffice to save all readers from similar difficulty. No less than three chapters are devoted to treatment, and in these the value of internal remedies of epidermic, endermic, hypodermic medication, and of hygienic treatment, is fully discussed. Our author is no believer in specifics, but has great faith in the internal use of sulphurous acid in certain forms of dyspepsia associated with sciatica. The use of alcohol in some pure form is insisted on as most beneficial. Ether is mentioned, but has not been tried fully; we wish it had, as in our opinion it is preferable to alcohol in many cases, and is most valuable on account of its power of aiding the assimilation of fat, a substance most important in the nutrition of nervous tissue. Electricity is considered at length, and the remarks thereon are well worthy of attentive study, as, in fact, are all these chapters on treatment. "The true, and almost the only remedy for sciatica—the hypodermic injection of minute quantities of morphia"—is considered last. To this mode of treatment the author owes his own cure, and he dwells on it with a natural gratitude. All he writes on this part of his subject is expressed with a care and earnestness which carry authority, and the sixteen cases (including his own) which are appended bear the strongest testimony in favour of the efficacy of morphia thus administered. After describing the form of syringe which is preferred, a solution of ten grains of hydrochlorate of morphia dissolved in two drachms of water is recommended as the best. "This solution is always solid at ordinary winter temperature, and generally so in summer. It contains half a grain of morphia in every six minims, and must be heated before each injection." The solidity has two advantages: it prevents decomposition and obviates mistakes in using the solution. Two minims (one sixth of a grain of morphia) is the dose recommended at first, and very careful directions are given as to the mode of injection, and the effects to be expected. At first, one injection in the twenty-four hours is considered sufficient; this may be followed by a larger dose, and in severe cases two or even three injections may be given daily. The practice of giving more than half to three quarters of a grain of morphia at one application is strongly objected to as far as regards sciatica. Hypodermic morphia when well borne generally increases the appetite, and thus the patient's general health is improved by the same means that remove the local pain. The number of injections required to effect a cure varies according to the severity and chronicity of the case. In some a few injections suffice, in others weeks or months of treatment may be necessary. In the graphic description of his own case the author says, "The injections had to be repeated nightly for many months. But this is certain, this is absolutely unquestionable: from the first application the patient improved." "In the whole range

of therapeutics I know of no instance where one remedy has produced so definite and wonderful a cure in so apparently hopeless and aggravated a case."

The chapters on lumbago and brachialgia are, we think, scarcely equal in merit to the earlier parts of the book, but both contain much useful information, and the cases recorded show strongly the advantage of the mode of treatment adopted.

The last chapter is a criticism of the views experienced by other writers. Throughout it there runs a healthy contempt for the visionary speculations on the origin of neuralgia, in which some recent authors have indulged. Good honest doubt is a feeling which cannot be too frequently expressed by medical writers in this age when the tendency to theorise is almost unchecked by the necessary severity in criticism. We congratulate Dr. Lawson on having boldly attacked this tendency, and we heartily recommend his book not only for its great practical value and pleasant style, but also for the wholesome absence of wild hypothesis.

Cobbold on Worms.[1]—The subject of internal parasitism is daily becoming more important in some of its aspects, and the prevalence of parasitic diseases is likely to increase with the development of our foreign meat supply. On this account the little book before us is welcome, coming as it does from one who has devoted himself most earnestly to the study of helminthology, and added much to our knowledge. Dr. Cobbold has hitherto been known rather as an enthusiastic naturalist than as a physician, but now comes before the profession in the new character of a practitioner, writing on the morbid conditions which he has specially studied.

The twenty lectures of which the book consists were delivered at the Middlesex Hospital, and give a short and clear account of the symptoms and treatment of the several intestinal worms. Trichiniasis, the *Bilharzia hæmatobia*, and Pseudhelminths are also considered.

Tapeworms occupy the first and larger portion of the book, and the symptoms produced by them and the means by which they are to be expelled are discussed at length. No less than forty-two cases of tænia are quoted in illustration of the author's views. The remedy he most generally relies on is the extract of male fern, but areca-nut powder, kousso, and other remedies are occasionally prescribed, some cases well illustrating the fact that when one remedy fails another will sometimes succeed in dislodging the invader. In his practice Dr. Cobbold oftentimes discountenances immediate treatment, especially when a portion of a tapeworm has been recently got

[1] *Worms.* A Series of Lectures on Practical Helminthology, delivered at the Medical College of the Middlesex Hospital; with Cases illustrating the Symptoms, Diagnosis, and Treatment of Internal Parasitic Diseases. By T. SPENCER COBBOLD, M.D., F.R.S., &c. &c. London, 1872.

rid of. Drugs under these circumstances, he says, have "no chance of giving thorough evidence of their efficacy," and he therefore prefers to wait some weeks, or even months, till the worm again arrives at maturity, and the proglottides reappear in the evacuations. The rule appears to be, the more you have to operate on the more likely you are to succeed. Of course Dr. Cobbold recognises the necessity of immediate treatment in many cases. For round worms, the frequency of which appears to us to be underrated, the one great remedy is santonin. To illustrate its efficacy we are told how "a man passing through the laboratory of a pharmaceutical firm in the City picked up a santonin lozenge and swallowed it. During the following night or morning, to the astonishment of his wife, he passed a huge lumbricus, which had thus evidently been expelled by the action of a single grain of this valuable drug. The man had never complained of parasitism of any kind, nor was he aware of the medicinal character of the sweetmeat."

In the chapters on threadworms many useful hints on treatment will be found; it seems to us, however, that there is scarcely enough stress laid upon local treatment in these cases as compared with the internal administration of drugs. It certainly seems a roundabout way to give a medicine by the mouth to kill a parasite in the large intestine, often in the rectum, and we think many of the remedies recommended might be used more suitably by injection. We do not wish it to be understood from this that Dr. Cobbold ignores local treatment in these cases; on the contrary, he devotes much space to it, but he has, we think, hardly suggested as many local medications as he might have done.

The cases in which the book abounds will well repay perusal; in them the student and practitioner will find many a useful suggestion. They would have been still more valuable if formulæ had been given more frequently, and if the symptomatology had been more fully described. To the class of readers for which this book is intended such detailed information is most useful, and the mode of prescribing the more uncommon remedies is almost essential. At the end of the book there is a very copious index, which adds to the usefulness of this very practical and useful little volume. In an appendix there is a record of the author's contributions to the science of helminthology. This has been added, because some authors on this side of the channel appear to have systematically ignored Dr. Cobbold's labours. These "puerile negations of home work" are fully answered by the appendix, but we cannot help thinking it was hardly worth while to have printed a list extending over five pages to confute such insignificant adversaries.

Roscoe's Chemistry Primer.[1]—This little book appears most

[1] *Science Primer—Chemistry.* By Professor Roscoe. London, 1872. Pp. 104.

opportunely. Those schools into which the elements of natural science are at last to be introduced will derive great benefits from the Science Primers now being published by Messrs. Macmillan. The idea that elementary knowledge could be as well imparted by teachers who knew but little as by those who were well read in their subjects has happily been abandoned; and it is particularly gratifying to find that an eminent chemist like Professor Roscoe can condescend to childish capacities, explaining with accuracy and simplicity some of the rudimentary phenomena and facts relating to fire, air, water, and earth.

The young student is here introduced to the science of chemistry in the most effective as well as most fascinating way. Very common materials are taken as the subjects of the experiments, so that the outward aspect of the substances employed is already perfectly familiar. Their chemical nature is explained by easy stages as the results of each experiment are tested and unfolded. Well-drawn pictures of the apparatus used and its arrangement are given in a series of neat woodcuts, while at the end of the volume a few of the most important chemical principles are argued out with great clearness from the starting-point of the experiments previously described. We are not quite sure that the author has adopted a satisfactory plan of dealing with chemical names. He calls CO_2 carbonic acid, for instance, and talks of oxide of copper and of calcium chloride. In a table of common and chemical names (p. 85) he speaks of red lead oxide as the chemical name of red lead, and states that sugar of lead contains lead and acetic acid. As Professor Roscoe adopts the ordinary use of the term acid (p. 65), it is a pity that he does not adhere consistently to this usage in every part of the book before us.

Watts's Dictionary of Chemistry.[1]—It is satisfactory to find that the indefatigable Mr. Watts has been encouraged by the favorable reception of his great work to bring out the present *Supplement*, bringing the record of the progress of the science down to the end of 1869, and including even some later additions to our knowledge. The fifth and final volume of the 'Dictionary' appeared in 1868, but the usefulness of the original will be maintained, and even enhanced, if adequate supplements are issued from time to time.

The present volume is interesting and well written. We see that Mr. Watts himself has written the largest portion of it, and it is clear that he has executed his difficult and varied task with great skill. Of the important articles on electricity and

[1] *A Dictionary of Chemistry*. By H. WATTS. Supplement. London, 1872. Pp. xiii and 1136.

heat, by Professor Carey Foster, it would be difficult to speak too highly; while Professor Roscoe's essays on the chemical action of light and the methods and results of spectral analysis give a concise and clear notion of the recent advances in these important branches of research. Dr. Michael Foster's article on the proteids gives a summary of our present knowledge of the important albuminoid substances which occur in animals and plants. Yet it must be confessed that an immense amount of analytical study of these perplexing "flesh-formers" is requisite before their relation and transformations can be understood, or a satisfactory classification of their varieties can be devised. A very large part of the new matter relating to hydrocarbons, in this supplementary volume, is concerned with the recent development of the benzol series and its derivatives. The theoretical views now in vogue as to the constitution of the aromatic hydrocarbons and the products derived from them are expounded with singular clearness by Mr. Watts. The important discoveries in the synthetical formation of substances of animal or vegetable origin are duly recorded and explained. We find an account of the methods of preparing alizarine, the colouring principle of madder, and of coniine the alkaloid of hemlock, together with numerous other researches of similar importance.

If there be any defects in this supplementary volume, it is rather hard to find them. But we may venture to suggest that the first five letters of the alphabet came in for the lion's share, occupying no less a space than 660 out of 1136 pages; and we think that the information on the important subject of mineralogical chemistry should have been more complete, more recent, and more exact. Several new minerals are left altogether unnoticed, while the accounts given of some species of recent discovery convey no accurate idea of their real nature. An error or two as to date has crept into this section, as, for instance, where, on pages 1030 and 1135, the discovery of the extraordinary absorption bands of certain zircons, first announced by Professor Church in 1866, is attributed to Mr. Sorby, whose observations did not appear till 1869.

We trust that Mr. Watts will make his next supplementary volume a still richer storehouse of information than that now before us, and this result, we think, may be accomplished by securing a larger number of contributors. In the original Dictionary the help of fifteen chemists enabled the editor to represent adequately the position of the several departments of the science; the present supplement is the work of six only.

Yearbook of Pharmacy for 1871.[1]—This volume merits our warmest commendation; it is quite equal, as to execution and contents, to the year book for 1870, which we favorably noticed in these pages. Pharmaceutists are evidently alive to the importance of the thorough study, both scientific and practical, of the materials and methods for preparing medicinal agents. One of the best evidences which can be adduced of this satisfactory condition of things is to be gathered from the yearly list of researches suggested by the Pharmaceutical Conference, the report of this association forming part of the volume before us. This list indicates the special directions which it is desirable that pharmaceutical investigations should take, and contains many suggestions worthy the attention of the scientific chemist. The 'Year Book' reports many results of such researches, and gives as well an admirable *résumé* of the recent progress of our knowledge of materia medica, pharmaceutical chemistry and pharmacy, both in Europe and America.

Stewart's Science Primer—Physics.[2]—Insignificant as this book may seem, we look upon its appearance just now with very considerable satisfaction. If the rudiments of natural philosophy are to be taught to children they should be taught well: nothing will conduce to this result more effectively than the use of such a sound little book of elementary notions as that now before us. We do not often review works on physics in these pages, but the present occasion seems to be an opportune one for a critical study of the way of teaching the subject to which this 'Science Primer' is devoted. How much more easy and how much more satisfactory would the special scientific studies of the medical student be if he had thoroughly mastered in his schoolboy days the contents of Professor Roscoe's 'Chemical Primer,' and that on 'Physics' by Professor Balfour Stewart!

The introduction to the book before us assumes that the 'Chemical Primer' has been previously used. But the knowledge of chemistry required for the comprehension of Professor Stewart's little book is so limited, that the study of both primers might well proceed together, or, if a choice be made, the 'Physics Primer' might advantageously be mastered first. In point of fact, the common phenomena of heat, light, sound, gravity, and electricity are, in a measure, observed by every intelligent lad, and so he is likely to feel more interest in the explanation of the interior meanings of these ordinary facts of

[1] *Yearbook of Pharmacy for* 1871. London. Pp. 640.
[2] *Science Primer—Physics.* By BALFOUR STEWART. London, 1872. pp. 135.

nature than in those of the chemical changes which he observes very often for the first time in the lecture-room.

The language of this primer is easy, clear, and, for the most part, precise; the general arrangement of the matter is appropriate to the object in view, while nothing is touched without some real knowledge being imparted concerning it. Perhaps more might have been attempted as to light, since the pupil will have already accustomed his mind to the idea of molecular motion in the case of heat and sound. It is very satisfactory to find, however, that there is nothing in this book which the student will have to unlearn.

This primer will serve two ends, constituting a text-book for the learner and a guide to the lessons of the teacher, who is expected to perform the experiments described in it, and to give the principles of the science in the language of the text-book, from which, indeed, he can hardly depart with advantage. Additional matter will be introduced rather in answer to those questions which, we will hope, the pupils will be encouraged to ask; or when the teacher sees that his pupils are in a fog without being able to express their difficulty in a formal question. Having premised that this primer is, on the whole, a very excellent little book, we venture to offer a few criticisms upon it, from both the pupil's and the teacher's point of view.

In a book intended to be used by children, it is necessary that the illustrations should be accurate, and it would be all the better if they were well drawn. A picture of a piece of apparatus makes a far more lasting impression on the mind of a child than the most elaborate letterpress description. We cannot say much in praise either of the execution or of the accuracy of the illustrations of the 'Physics Primer;' all are badly drawn, and many give erroneous ideas. In fig. 2 the first string should not remain attached to the plate; it would have been an improvement had the figure shown two or three pegs passing through the plate, which might rest in two loops fastened to the same string. This plan would determine the position of the centre of gravity with tolerable accuracy. Fig. 8 might have shown the position and action of the valves of the forcing-pump, and of the collar which surrounds the larger piston. Fig. 20 represents an almost worthless instrument, and represents it badly; one would imagine that the water level was an instrument of gigantic size. It would have been far more *useful* to have figured and described the ordinary spirit level, showing the curvature of the tube. But the most serious errors are in figures 17 and 18, the former of an air pump, the latter of a water pump; in fig. 17 both valves are open, and as if to confirm the wrong impression the child gets, the mistake

is carefully repeated in fig. 18. We might have supposed that they were thus drawn to show their position, but in that case there would have been a note in the text or a footnote to that effect. The only sentence that might seem to the child to refer to the point is, "Both valves open upwards and not downwards," which rather helps the delusion. It is well in fig. 37 that the word "lens" is printed near the two parallel lines in front of the sliding tube (which is represented as rigidly fixed) of the camera—for no one would imagine it to be anything but a plane piece holding its position in spite of the laws of gravity. At a time when French works on physics are rapidly gaining popularity in this country chiefly because of the beauty and accuracy of their illustrations, it is a pity that a book in many respects so really good as this should not have a fair chance of the success it deserves, just because of the battered and inaccurate diagrams which disfigure its pages.

The teacher might fairly object to the wording in places. The definition of motion (Sect. 2) is far from clear; one objection an intelligent boy would at once make to it is allowed in Sect. 40 by a reference to the very thing the boy would think of—his top. To get any idea of motion it is necessary first to think of something fixed with reference to which the motion takes place; in fact, all motion is *relative*, and the sooner the student has this idea clear in his mind the better it will be for him in his subsequent physical studies. In Sect. 14 the meaning of the word size is unnecessarily restricted. In Sect. 32 the introduction of the word "therefore" is certainly confusing, and all reference to weather generally might have been omitted, as few teachers would be able to give satisfactory answers to the host of questions to which the assertion of this article will be sure to give rise. We cannot see why the principle of the syphon is so difficult that the child could not understand it; the action of a syphon is so striking to a child that it would hardly be content with the information contained in Sect. 35. The statement about an echo in Sect. 46, that "in this particular case it will come back along the very same line that it went," is far from being necessarily true. A teacher would explain that the cliffs are supposed to form a cylinder round the observer, who is in the axis; but if the child is reading the book by itself, and we have no doubt that this will often be the case, this necessary limitation would be unwise. It does not seem to us that any clearness of description is gained by talking of squeezing lines against a surface; in what direction is the squeezing to take place? Better to have introduced the normal at once with the ordinary statement of the law of reflection. The law of refraction is not referred to.

It is with real pleasure that we refer to the introduction of the principle of energy into this book. It is the foundation of all physical science, and cannot be taught too soon; the author has succeeded in this good move admirably; the definitions are clear and precise, and the foundations of a sound philosophical training are thus laid—and well laid.

Turning from the subject-matter, we regret to see the remarks about apparatus in the preface, and the description of the same at the end of the book. Incalculable benefit will be conferred on young students if they are taught how easy it is to contrive a great deal of the apparatus required for their early experiments. We have seen mere lads for a few shillings make a glass pump, an electric machine, galvanic battery, and similar pieces of apparatus. We are convinced that for about ten pounds *all* the apparatus necessary for illustrating all that is contained in his primer might be obtained, and with very much greater profit to the student if with less to the instrument-maker. To go into the method of making pieces of apparatus would, of course, make the primer too large, but at least let us not prompt the pupil to have immediate recourse to the instrument-maker. A few hints on the use of glass tubes, corks, wire, tin foil, leaden bullets, &c., would in a future edition replace with advantage the three or four pages to which we are now referring.

We have reviewed this primer somewhat at length because we are thoroughly convinced of the importance of the series of which it is one. "Give us the child, and we will be answerable for the man," is even more true of science than it is of religion. We hail the appearance of these primers with satisfaction, and see in their adoption the promise of a rich harvest of well-trained scientific men.

MacCormac on Consumption.[1]—Dr. MacCormac has long been known as the advocate of a somewhat odd hypothesis concerning the production of pulmonary consumption. To him tubercle is nothing more than "the dead unoxidized carbon of the living organism," and its cause the respiration of pre-breathed air. At page 16 of the book before us he thus writes:

"Other than such fouled and halting air, coupled with its habitual inhalation, there is no actual or possible source of phthisis, and I believe in none. The detritus of degradation, speaking of the dead unoxidized carbon, remains under such circumstances more or less within, instead of being thrust beyond the precincts of the living breathing organism. The only means of explaining what

[1] *Consumption and the Breath Rebreathed. Being a sequel to the Author's Treatise on Consumption.* By HENRY MACCORMAC, M.D., Consulting Physician to the Belfast General Hospital, &c., &c. London, 1872.

becomes of it resides in assuming its conversion into, and, in fact, identity with tubercle. The effete unoxidized carbon, then, is tubercle, while, conversely, tubercle itself is no other than the effete unoxidised carbon. One condition of things, to wit, the habitual respiration of already breathed air having begun, the sequence of the other, namely, the deposit of tubercle, follows as a matter of necessity. Reciprocally, and reciprocally they explain and account for each other perfectly."

Such is the author's explanation, and such the gigantic assumption on which it rests. From the first page to the last this hypothesis is stated over and over again, almost as if the writer fancied it might gain some appearance of truth by frequent repetition. We have quoted the passage given above because it is a good sample of the style of writing and reasoning, and of the perfect self-satisfaction which characterise the book.

The wide reading of the author has, it is true, enabled him to collect a large number of facts, which give some countenance to his views, and these are brought forward with a skill and persistency which win our respect if they cannot command our assent. The fact is the book is the production of an enthusiast, and on this account only can we pardon the manner in which the work of such men as Niemeyer, Lebert, Waldenburg, and Andrew Clark is spoken of.

The truth of his own hypothesis appears to have become so fixed an idea in Dr. MacCormac's mind, that he can tolerate no other explanation of tubercle, but, like a Don Quixote of Science, conscientiously attacks all who differ from him.

Although the book is disfigured by these blemishes, it has a redeeming feature in its earnest and, at times, even eloquent advocacy of the importance of breathing pure air. Free ventilation is one of the greatest wants of modern houses; and we cordially agree with Dr. MacCormac in believing that open windows and out-door life are the best preventives of tubercular disease, differ we never so widely from his explanation of the fact.

As a vigorous protest in favour of the importance of hygienic measures in preventing lung affections the book may be useful. In other respects we regard it as a curiosity—curious alike in argument, speculation, and style.

Wolff on Zymotic Diseases.—It is a painful but necessary duty to point out by adverse criticism the defects in a scientific work, especially when the subject, like that of "zymotic dis-

[1] *Zymotic Diseases: their Correlation and Causation.* By A. WOLFF, F.R.C.S. London, 1872.

eases," is one which is at present pervading and irritating the public mind as well as that of the profession. We would gladly welcome any publication having the aim of that before us, viz., to simplify this complex subject, and to discover a clue to explaining all the phenomena connected with zymotic diseases, provided the premises on which such treatise is based are correct and the reasoning it contains is consistent.

Mr. Wolff appears to us to have written his work by starting with a fixed idea, to which he endeavours to reconcile well-known pathological facts and the characteristic symptoms of zymotic diseases. He truly remarks, at the commencement of his work, that most of the views expressed in it "may be found scattered in the writings of various medical authors;" while his descriptions of symptoms are elementary, and as appropriately remarked "have no pretension to be considered as finished pictures." Hence his work cannot claim originality, nor is it based on careful clinical observation and research. It contains many inaccuracies, as, for example, when it is stated in page 71 that "the pustules that are observed in the vicinity of a dissection wound, the pustules consequent on irritation by croton oil or tartarised antimony, cannot be distinguished from the pustules of smallpox. If the contents be placed under the microscope, no observer could possibly distinguish between them, and the most minute chemical analysis has failed to detect any product peculiar to smallpox pustulation." It is much to be regretted that the medical press is inundated with works which confer no benefit except the title of author. The "sweet simplicity" exhibited in the following sentence recommends it as a fit conclusion to this notice of Mr. Wolff's work 'On Zymotic Diseases:'—"Whatever may be the cause (?), the disease being once set up, the effect, varying in accordance with the different structures primarily affected (?), resolves itself into a uniform process of structural irritation, structural inflammation, and structural death."

Glenn's Laws affecting Medical Men.[1]—This is a well-arranged book of reference on all the laws affecting the qualifications, duties, responsibilities, and status of medical men. Its publication has followed very speedily on the work of Mr. Weightman, 'The Medical Practitioner's Legal Guide,' which we last year commended to our readers. Necessarily the subject-matter of the two volumes is the same, but it seems to us that Mr. Glenn has subdivided it better, and made a more readable book, and one more easy to refer to. At the same time he has not given

[1] *A Manual of the Laws affecting Medical Men.* By ROBERT G. GLENN, LL.B., Barrister-at-Law. London, 1871. Pp. 460.

the text of the law nearly so fully as Mr. Weightman has done, but has contented himself with summarising the several enactments he has had to deal with.

Generally speaking, a summary of the law suffices for the non-legal inquirer, who will mostly show more wisdom in referring a debateable matter to a competent legal adviser than in attempting to extract the meaning of the law for himself.

There are some legal practices affecting the duties and remuneration of medical men which much need emendation. We allude especially to those pursued in courts of law where medical men are called as witnesses. The scale of fees laid down is most inadequate, and in reference to civil causes there is an entire want of a uniform system, or of well-understood usage in respect of fees to medical witnesses.

Some medical practitioners are very successful in securing for themselves a fair equivalent for their opinion, their trouble, and loss of time, whilst the majority are victimised from want of knowledge how to make previous arrangements with solicitors, and are made to accept the miserable allowance assigned by courts of law, and which is supposed to be the whole of what they can demand and all that will have the sanction of the taxing masters.

We have referred to both the treatises mentioned, but have not been enlightened as to the rights and privileges of medical men with regard to this matter. At least the information given is meagre, and conveys no more than that the various courts have a tariff of fees for medical witnesses, which fees are, generally speaking, very inadequate.

Works such as the one now under notice are most desirable for the instruction and guidance of medical men in all those relations in which they find themselves to stand with the state and with society, and should find a place in all medical libraries. Mr. Glenn's treatise is an excellent example of what such works should be.

Before closing this notice we would call attention to a chapter on medical etiquette, by Dr. Alfred Carpenter. It may be accepted as presenting generally the approved principles of conduct among medical men, but we are not prepared to accede to all his views. The attempt to place in a definite shape those principles is very praiseworthy, and we should be delighted to see his opinions well discussed, and as a result, to obtain something in the way of an accepted general code of rules.

Photographic Medical Periodicals.[1]—We place the foregoing

[1] 1. *Photographic Clinique of the British Hospital for Diseases of the Skin.* A Quarterly Periodical edited by BALMANNO SQUIRE, M.B., &c.

2. *Revue Photographique des Hôpitaux de Paris.* Bulletin Médical publié par BOURNEVILLE et A. DE MONTMEJA. Published monthly.

publications together as examples of the application of photography to illustrate such lesions in the living subject as can, by their position, become represented by means of that process. Those lesions are necessarily actually external, or otherwise are such as can give images by reflection capable of being thrown upon the sensitive receptive surface within the camera. As a clinical illustrator photography is therefore limited in its application, although it can, as a matter of course, be resorted to to picture any morbid products after separation from the body.

Mr. Squire has already successfully essayed to depict skin diseases by photography, and in the periodical just started he proposes a wider range, limited only by the supply of cases to illustrate those maladies. He has favoured us with a copy of the first part of this new venture, consisting of a large photograph of oval or true keloid, accompanied by descriptive letterpress presenting a notice of the two kinds of keloid, and also a history of the case from which the photograph is taken. He does not discuss the pathology of the affection, but restricts his matter to describing its outward appearances and symptoms. As a truthful reflection of nature the photograph is beyond the range of criticism. To make it look more natural it is slightly coloured, the tint of the keloid growth being duly brought out, excepting for a duskiness inseparable, we presume, from photographic pictures; unless colour be introduced so abundantly as to conceal the delicacy of the lines and shadows nature truthfully stamps on the picture.

We hope Mr. Squire will meet with such encouragement as to induce him to persevere with the publication.

The 'Photographic Review of the Hospitals of Paris' takes the widest range of subjects to which photography is applicable in the living subject. It is not a new undertaking, but has reached its fourth year.

Three photographs are contained in each number, and are used in illustration of facts detailed in some clinical lecture at one or other hospital, or in some communication published in the accompanying pages. But over and above the articles connected with the photographs there are extracts on various branches of medicine and surgery from other periodicals, reviews of books, and other contributions common to other journals.

The class of subjects illustrated will be best understood by quoting the photographs to be found in several numbers. Those contained in the January (1872) number represent the appearance of the limbs in cases of infantile paralysis; those in the February number respectively necrosis of the lower jaw, coloboma of the choroid, and congenital torticollis; in the part for March, congenital torticollis and permanent contracture of the

flexors of the three inner fingers; in that for April, two examples of encephalocele and one of a calcified chondroma; and in that for May last a case of chondro-sarcoma of the femur, an illustration of tubercle of the testicles, and a specimen of pseudo-arthrosis of the leg.

The lesions thus illustrated are sufficiently interesting in themselves, and we may assume that the selection made is dictated in some measure by a desire to pourtray those of rarer sorts, as well as by the circumstance that they have afforded matters for clinical comment by one or other eminent hospital physician or surgeon, whose remarks are worthy of record in the journal, even apart from the figures. We therefore look for merit both in the illustrations and in the text of this journal, and happily we are not disappointed. For example, the possessor of the photographs representing the sufferers with infantile paralysis has also the advantage of the observations of M. Charcot on that malady, together with a full description of a case by Drs. Ball and Lionville. So likewise the history and treatment of torticollis are dealt with by M. Broca.

Truthful delineations of morbid states, as presented by photography, convey with rapidity and ease instruction that would be less perfectly imparted by long descriptive accounts; and the photographic art has now so advanced that it can be applied most readily, whilst at the same time its results have, by modern processes, acquired a permanence that was much wanted at an earlier period of its progress. We anticipate a wide extension in the application of photography to illustrations as well in other works as in medical. Where the art is practised by means of a sufficient lens, the result is beyond criticism in reference to its truthfulness and in this particular excels any production of the engraver, who is naturally and inevitably not a mere copyist, but a maker of a picture, and who, with perfectly honest intentions, is led, it may be even unwittingly, to introduce lines and shadows and accessory minutiæ, by way of embellishing his work, and so departs more or less from the object he has undertaken to portray.

It is a credit to our neighbours across the channel to have given that measure of encouragement to this 'Photographic Review' as to enable its editors and publishers to carry it on for four years. We hope that encouragement will be continued and extended.

Examination Questions.[1]—The author, with a laudable desire to help students in testing their knowledge of what they have read, has compiled this small book of questions on the various

[1] *Examination Questions.* Arranged by H. Aubrey Husband, M.D., &c.

subjects required by the medical examining boards. Most of them, it appears, have at one one time or other been set in examination papers. The student who can answer them should be indulgently treated by the examiners, and, in due recognition of the help received from this little book, should recommend other aspirants for medical licenses to possess themselves of a copy.

Notes on New Editions.

Manual of Zoology.[1]—The author of this manual is one of the most successful writers on Zoological Science of the day, and his various works thereon, addressed to more and to less advanced students, have been fully appreciated as they well merited. The first edition of the present manual was criticised in the October number of this Review, 1870; the present one has been carefully revised and all the more striking discoveries of recent date have been noticed. Additions have been especially made in the zoology of the Vertebrata. The work will, therefore, still maintain its position as the Manual of Zoology best adapted to the wants of students, both by being well brought up to the standard of knowledge of the time, and by the clearness and succinctness of its descriptions.

Elements of Chemistry: Theoretical and Practical.[2]—When a scientific work of the magnitude of Prof. Miller's Chemistry runs on to a fifth edition, such a mark of public approval puts the critic out of court; at least so far as concerns the expression of an adverse opinion of its merits, and leaves him nought to do save to bear witness to the due revision of its subject-matter so as to make it truly representative of the progress the science has made. By the lamented death of Dr. Miller the work of revision of the first volume has devolved on Mr. McLeod, the professor of experimental science at the Indian Civil Engineering College, in whom the fullest reliance can be placed for the performance of the task. He has, however, avoided unnecessary alteration of the edition of 1867, which conveyed an accurate knowledge of chemical physics as understood at that date, and to which (although in general physics there has been much advance), no such extensive additions have been made. The most important additions are on the subjects of solar chemistry and the theory of atomicity. Professor McLeod has pursued a wise course in not expanding the work in the direction of general

[1] *Manual of Zoology.* By H. Alleyne Nicholson. Second Edition.

[2] *Elements of Chemistry: Theoretical and Practical.* By W. Allen Miller, M.D., &c., Revised by Herbert McLeod, F.C.S., &c. Part I, Chemical Physics. Fifth Edition, with Additions.

physics; for by so doing he would have lessened its value to medical and chemical students, whose range of reading is now-a-day sufficiently wide without further expansion in the region of collateral sciences. Indeed to 'get up' this one volume is a task not to be extorted from every medical student.

Dobell on Winter Cough.[1]—The first edition of this volume chiefly consisted of a short course of lectures given at the City Road Chest Hospital, and presented the results of the author's practical experience of bronchitis, and his creed respecting the pathology of pulmonary emphysema. In regard to this just-named pulmonary lesion he is a firm believer in the expiratory hypothesis, and admits no other explanation of its production. Moreover, throughout the treatise he makes much of an emphysematous condition, as if complicating most cases of bronchitis —an untenable assumption.

But to return to the character of this second edition. The author tells us in his preface that the first edition has been long out of print; that his professional engagements interfered with the preparation of a new edition, particularly as he desired the new issue should be something more than a reprint. So, he adds, he has done his best to thoroughly revise the text, and he proceeds to give the list of new matters introduced in the second edition. However, when we come to examine the work we are disappointed with the amount of revision effected, for we find the lectures contained in the first edition reproduced almost verbatim, and no further clinical experience referred to in elucidation of the teaching put forward. Here and there a sentence or a paragraph or two are introduced, and all the more considerable additions are fully enumerated in the preface; to wit,—two new Lectures—one "On the Natural Course of Neglected Winter Cough, and on the Interdependence of Winter Cough with other Diseases;" the other "On Change of Climate in Winter Cough." Fresh matter has been inserted under the heads of "Ear Cough," "Post-nasal Catarrh," "Laryngoscopy," "Artificial Respiration," and "New Instruments and Methods of treating Emphysema of the Lungs." "In addition to this I have introduced coloured plates of some of the principal pathological conditions met with in winter cough, and to these I have appended concise statements of the signs by which they are indicated during life. Finally, I have given a complete index prepared by the best index-maker in London."

These additions, so catalogued, would indicate changes something more considerable than is actually the case. The former of the new lectures added occupies but ten pages, and the latter

[1] *On Winter-Cough.* By HORACE DOBELL, M.D., &c. New and Enlarged Edition.

only nineteen pages, of which much is borrowed matter from writers on climate; its sources are, however, rightly enough stated. The section on Ear-cough is an abstract of a paper read by Dr. C. B. Fox, of Scarborough, prefaced by a short history of a case; and the account of post-nasal catarrh has, in this second edition, been simply transferred from its place in the appendix in the first issue to the body of the work, with a few verbal additions, and represents no really new matter as is implied. What is said of Laryngoscopy is comprised in half a page, and amounts to no more than the assertion that this means of diagnosis is of no value in winter cough. The dissertation on artificial respiration occupies not very much more space, and is mainly a reproduction of a "clinical note" published in the 'British Medical Journal' in 1869. From this same periodical he extracts verbatim more or less of the account referred to of the "New Instruments and Methods of treating Emphysema of the Lungs," and consequently, as in the case of preceding additions noted, he has imposed upon himself little more than the use of scissors and the judicious interpolation of matter he has at some previous time written. As to the coloured plates they are two in number, each containing two figures, which so far as they go are satisfactory enough, though representing a modicum of the lesions connected with winter cough.

From this review of the new matter inserted in this second edition we cannot fully realise the difficulty Dr. Dobell has had to struggle against in its production, and we consider he has unduly raised the expectation of his readers with regard to the added matter. His subject smacks rather of popular than of scientific medicine; for it is one of purely symptomatic importance, and the lesions on which winter cough depends are but partially and one-sidedly discussed. Nevertheless if it offer no features to attract and satisfy the thorough pathologist, the treatise contains much both attractive and instructive to the practitioner, and is written in an easy colloquial style, which is a further recommendation.

A Guide to the Examination of the Urine.[1]—That Dr. Legg has succeeded in producing a guide to the examination of the urine highly satisfactory to the clinical clerks and students, to whom it is specially addressed, may be taken as demonstrated by the rapid appearance of one edition of his small treatise after another. Indeed, the book gives just that information, and just so much of it, as the working hospital student needs to aid him in the diagnosis of urinary diseases. It would be spoiled by endea-

[1] *A Guide to the Examination of the Urine.* By J. Wickham Legg, M.D. Third Edition. 1872.

vouring to make the teachings more exhaustive, and we trust Dr. Legg will not be tempted on such a course. He has hitherto avoided it, and this third edition is, as he tells us, "in nearly all important respects, the same as the second," some paragraphs only having been more expanded to illustrate what is the clinical value of the morbid changes found in the urine. As a concluding note in its praise, we may remark on the more convenient size of this last edition to that of the first, for carrying in the pocket

Smith's Handbook of Dental Anatomy and Surgery.[1] — Dr. Smith's 'Handbook' has been well received, and has now appeared in a second edition. This may be taken as evidence that its subject-matter is right in quality, and meets a demand. Except for such evidence we should have inferred that it contained a great deal of matter which might have been left out without disadvantage; that, for instance, students and practitioners who, as assumed in the preface, have made "some advance in the usual branches of medical education," would not have required set before them the rudimentary rough anatomy and physiology of the teeth and maxillæ, which occupies one third of the book. On the other hand, again, we should have argued that such students and practitioners who referred to this book, considered as a special treatise, would have liked to have found a fuller history of diseases of the teeth, gums, and jaws, and a more complete revelation of the art and mysteries of dentistry than are to be found in its pages.

However, we feel we are reckoning without our host, having no dental practitioner at our elbow to enlighten us as to the wants and wishes of those actually in, or of those seeking to enter in his speciality; and that, indeed, in face of the demand for a second edition of this treatise, we are verily guilty of misjudging on those points. Wherefore let it be concluded that Dr. Smith is practically right and that we are theoretically wrong in the matter of the contents of his book; but with the proviso kindly allowed to those who are persuaded against their will. Moreover, that no injustice be done to the author, we would bear witness to the correctness of the instruction he has undertaken to give, and signify our satisfaction that it has been so well appreciated.

[1] *Handbook of Dental Anatomy and Surgery for the Use of Students and Practitioners.* By JOHN SMITH, M.D., &c.

Original Communications.

I.—**Statistics of Mortality among Prisoners**: being an Inquiry into the Death-rate of the Government Prisons of England, its Causes, and the Circumstances affecting it; with Pathological Observations. By Dav. Nicolson, M.B. (Aberd.), C.M., Assistant-Surgeon to Her Majesty's Invalid Prison, Woking.

The question of mortality in convict prisons cannot be settled by an appeal to mere figures. I propose, therefore, to consider the subject under the following heads:

I. Brief outline; classifying the prisons and glancing at the *régime* implied in 'penal servitude.'

II. Classes from which the prison ranks are recruited:
1. The social status of recruits.
2. Their physical and mental condition.

III. General statistics of convict population and mortality.

IV. Statistics and pathological remarks on the various causes of death among prisoners.

V. Has the mortality of convict prisons increased or decreased?

VI. Concluding remarks.

I. Brief outline of prisons and 'penal servitude.'

The Government prisons of England are eleven in number, and all are situated either in London or in the southern counties. The metropolitan ones are Pentonville, Millbank, Brixton, and Fulham.

The following table gives particulars:

TABLE I.—*Showing the distribution and nature of convict prisons of England, with the average population, for* 1870.

Nature of Prison.		Name.	Situation.	Average population for 1870.
Male Prisons	Separate or reception prisons	Pentonville	London	687
		Millbank	Ditto	362
	Public works	Portland	Dorset	1568
		Chatham	Kent	1586
		Portsmouth	Hants	1218
	Light labour and invalid prisons	Brixton	London	486
		Parkhurst	Isle of Wight	468
		Dartmoor	Devon	881
		Woking	Surrey	684
Female prisons		Millbank	London	350
		Woking	Surrey	706
		Fulham	London	134

Millbank alone contains prisoners of both sexes. The prisons at Woking are distinct establishments, the one being the general prison for females, and the other being for the most invalid class of males. Broadmoor is the asylum for insane criminals, but it is under a different board of directors.

A prisoner on being sentenced to penal servitude is removed from the county or borough gaol to one of the reception prisons for convicts in London, where he undergoes what is termed his 'separate' confinement, extending over about nine months. He is then transferred to public works, where, under ordinary circumstances, he finishes his sentence. If, at the end of his separate confinement, or during any portion of his imprisonment, he is found to be unfit for the full discipline and labour of penal servitude, he is drafted off to some prison set apart for invalids or for the light-labour class of men. Thus, in a general way, the circumstances of the prisoner, *quoad* location, discipline, and labour, are made to depend upon his physical (and, when necessary, his mental) condition. The employment at public works consists, for the most part, of good steady labouring work—quarrying, excavating, building, brickmaking, &c. All this involves a good deal of artisan work, and at this the prisoners are employed as well, some being smiths, carpenters, masons, moulders, &c., and others painters, shoemakers, and the like. The lighter forms of employment comprise mostly farm agricultural work, tailoring, and oakum picking, with a hundred jobs of an incidental nature.

In the days of transportation the able-bodied only were selected and sent across the seas.

II. Classes from which prisoners are recruited.

1. The social status of recruits.
2. Their physical and mental condition.

1. All grades of society, from the Member of 'the House' down to the wretched beggar in the streets, are found here 'doing their terms.' The mass of prisoners undoubtedly belong to the lower strata of individuals who style themselves variously 'labourers,' 'hawkers,' and 'of no occupation,' men who never slip a chance of turning a *dis*honest penny, and who seem simply to live and trade in crime. Above these, in considerable numbers, comes the, perhaps, 'hitherto honest' man, artisan, or tradesman, who becomes malicious, or who, with a 'drop o' drink' in him, or in a fit of passion, deals an unfortunate and fatal blow at his wife or his fellow, or who, in the hour of temptation, gives way and steals his master's goods. Higher up still are to be distinguished the more limited number of the 'eminently respectable' class, who wear black coats, clean fronts, and go to church regularly; 'men of business,' these —needy clerks and peccant city men, with a sprinkling of the learned and professional—who are caught 'borrowing' money on highly irregular principles, or whose peculiarity consists in an unfortunate facility with the pen and a weakness for signing other people's names with it.

No matter to which of these grades the individuals belong, they are all put on the same footing in the system of penal servitude for which crime is the qualification.

2. Physical and mental state. This is a subject which it will be necessary for us constantly to bear in mind, and we must, therefore, obtain some clear notion upon it before starting. I believe that, as a class, prisoners are essentially *ill-conditioned*. Taking, first, the lower sections of the criminal class, they will be found to include men naturally of a low grade of intellectual and moral development, as indicated by their habits; and where the mental constitution is defective, it is no uncommon thing to find, side by side with it, some defects in the *physique*. Others, again, have the bodily constitution naturally weak, or they are the subjects of some positive hereditary taint. But besides this the mode of life of these people, and the circumstances attached to it, cannot fail to leave their impressions upon them in some form of disease or infirmity. From many of the invalid class we have their own confession that they have led what they call 'hard lives,' and that they have 'knocked about a deal in their time.' A 'hard life' and 'knocking about' are comprehensive terms, which represent in the aggregate a terrible history of drunkenness, debauchery, exposure, and irregularity, necessarily entailing great deterioration of the constitutional powers. On the other hand, the minds of criminals of the more

respectable order have been strained to the utmost in the ups and downs of speculation. The nervous system, unequal to the task imposed upon it, has become unhinged by worry and anxiety, to the neglect of the healthful wants of the body; gastric derangement is the consequence, and the heart or the brain becomes seriously involved. From all this it will certainly be no matter of surprise if we find that large numbers of our prisoners are *ill-conditioned*, either in body or mind. And what do figures tell us? In their Report for the year 1868 the directors of convict prisons state that, "by returns made in April, 1869, it appears that, of 5458 convicts who have passed the period when they were eligible for removal to public works, 1762 were invalids, or incapable to such an extent as to be fit only for light labour, and 162 were permanent invalids hardly capable of any labour at all; *i. e.* 5 out of every 14 prisoners (1 in 2·8) were invalids or fit only for light labour.

On the same authority (report for 1869) we find that as many as 370 males (almost the half) out of 784 received into Millbank were of weak and enfeebled constitutions on reception.

And Mr. Gover, the medical officer of Millbank, in his report for 1868, makes the following statement, which I give in full:—"The great majority of the prisoners who were removed to Dartmoor and Woking as invalids were suffering on reception from the diseases or infirmities marked as the cause of removal. Many of them improved very considerably in the interval between their reception and removal; and I may here observe, with reference to the population of the prison as a whole, that the general tendency to improvement, which has previously been a subject of remark, has been as manifest during the year just ended as in former years. Nevertheless, taking the population of this prison, exclusive of the imbeciles and epileptics, as affording a fair standard of comparison, I have reason to believe that the proportion of convicts afflicted with mental and bodily diseases is greater than would be found in any other section of the community."

And in his report for the same year Mr. Blaker, the medical officer of the Public Works Prison at Portland, says: "There can hardly be a doubt that the number of weakly and diseased prisoners is largely on the increase, and experience and observation lead me to think that the vast majority of these convicts entered prison suffering from latent or actual disease."

It is plain, then, that a large proportion (probably over one third) of criminals *bring with them* into prison impaired constitutions. This impairment or defect manifests itself, first, in the form of positive disease or infirmity; secondly, in a predisposition to certain forms of disease, mostly of a constitutional or phthisical nature; and, thirdly, in a want of ability to resist the destructive

influences of certain other diseases which are more active and local in their character.

III. General Statistics. — These are based upon the annual reports of the Directors of Convict Prisons, and embrace a period of fifteen years, beginning with 1856 and ending with 1870, the year last reported upon. The minor details of prison life, as bearing upon our present subject, have been during those years tolerably uniform; certain influential alterations in circumstances will be considered further on. The average annual population for the fifteen years, taking both sexes, was 7551; the males averaging 6419 and the females 1132.

Among the males the annual population ranged from 5862 to 7942. The numbers fluctuated more or less by a hundred or two from year to year until 1866, since which there has been a steady rise up to the maximum in 1870. This increase, I may state, is due, not to the number of convictions, but to the accumulation of convicts at home consequent upon the gradual cessation of transportation.

Among the females the numbers have varied a good deal. There was a pretty steady increase from the minimum, 810 in 1856, to the maximum, 1303 in 1863; subsequently the number fell considerably, but the returns for the last three years show a tendency towards an increase.

The proportion of females to males has been in the ratio of 1 to 5·67 for the whole period, or 3 females to 17 males. The cessation of transportation has, of course, caused an increase in the proportion of men latterly.

As to mortality, we find that during the fifteen years there were 1528 deaths (101 annually), of which 1289 took place among the males and 239 among the females. The average annual number of deaths per 1000 was 13·49; males 13·38, females 14·07; the range among the former being from 9·47 to 18·38, and among the latter from 6·60 to 21·19. The ratio of mortality among females, as compared to males, was as 1 to 5·36; or 3 females to 16 males.

TABLE II.—*Showing annual population, male and female, of convict prisons, with the mortality and its rate per* 1000, *for* 15 *years, ending with* 1870.

YEAR.	POPULATION.[1]			DEATHS.			DEATHS PER 1000.[2]		DEATHS PER 1000. M. AND F.	
	M.	F.	Total.	M.	F.	Total.	M.	F.	All causes.	Phthisis.
1856	6025	810	6835	91	15	106	15·10	18·51	15·5	7·3
1857	6650	922	7572	63	14	77	9·47	15·18	10·1	4·6
1858	6760	1043	7803	104	18	122	15·38	17·25	15·6	7·8
1859	6551	1189	7740	69	11	80	10·53	9·25	10·3	4·0
1860	6397	1269	7666	79	19	98	12·34	14·97	12·7	4·3
1861	6022	1205	7227	68	11	79	11·29	9·12	10·9	3·8
1862	5862	1211	7073	54	8	62	9·21	6·60	8·7	2·8
1863	6089	1303	7392	91	21	112	14·94	16·11	15·1	7·0
1864	6153	1274	7427	89	27	116	14·46	21·19	15·6	7·0
1865	5983	1248	7231	110	15	125	18·38	12·01	17·4	8·1
1866	5941	1017	6958	103	18	121	17·33	17·69	17·3	7·0
1867	6094	1008	7102	90	12	102	14·76	11·90	14·3	4·6
1868	6593	1104	7697	79	12	91	11·98	10·86	11·8	3·5
1869	7219	1185	8404	99	15	114	13·71	12·66	13·5	5·9
1870	7942	1190	9132	100	23	123	12·59	19·32	13·4	5·5
Aver. for 15 yrs.	6419	1132	7551	85·2	15·2	101	13·38	14·07	13·4	5·5

The average time to be passed in prison (deducting the proportionate remission) was, reckoned upon the sentences of the convicts in prison in December, 31st, 1868, for men five and a half years, for women five years. But probably this time will increase, for two reasons: first, owing to the greater length of sentences awarded, and, secondly, owing to a new regulation, by which the time remitted on a previous conviction is forfeited in the event of reconviction.

IV. Causes of Death; Statistics and Pathological Remarks.—We are now in a position to consider the classes or orders of disease to which this mortality is due; but I may first state that the striking features in the *post-mortem* examination of prisoners consist in the number of organs and tissues involved in the morbid processes, and in the extent to which particular organs have been destroyed. The extensive nature of the morbid appearances revealed after death among prisoners is explained, so far at least, by their originally unhealthy constitution, by their high rate of death from consumption, and by the frequency with which long-protracted fatal diseases in them tend to run into consumption.

[1] The sum of the daily average population of the various prisons.
[2] The deaths per 1000 are in all cases calculated upon the average population.

We shall take the causes of death in the following order:—

1. Diseases of brain and nervous system.
2. Diseases of respiratory system.
3. Diseases of circulatory system.
4. Diseases of digestive system.
5. Diseases of urinary system.
6. Constitutional and general diseases.
7. Accidents and violence.

Table III is constructed to show the actual and relative amount of mortality arising from the various causes during the fifteen years. They are mostly local; but a number are included under the term "constitutional and general," which are not referable to any particular system of the body. The column "zymotic diseases" is placed under merely for comparison, and does not form part of the Table itself, which already includes them.

TABLE III.—*Showing the distribution of mortality among the various causes of death, during* 15 *years, ending with* 1870.

Diseases.	Deaths in 15 Years.			Average Annual Mortality per 1000 Prisoners.			Per-centage of Deaths upon whole Mortality.		
	M.	F.	Total.	M.	F.	Both sexes.	M.	F.	Both sexes.
Brain and nervous system	113	26	139	1·17	1·53	1·22	8·76	10·87	9·09
Respiratory system — Phthisis	527	104	631	5·47	6·12	5·57	40·88	43·51	41·29
Respiratory system — Other diseases	155	18	173	1·60	1·06	1·52	12·02	7·53	11·32
Circulatory system	107	23	130	1·11	1·35	1·14	8·30	9·62	8·50
Digestive ditto .	108	25	133	1·12	1·47	1·17	8·37	10·46	8·70
Urinary ditto .	45	10	55	·47	·58	·48	3·49	4·18	3·59
Constitutional and general	161	32	193	1·67	1·88	1·70	12·41	13·38	12·59
Accidents and violence	73	1	74	·75	0·05	·65	5·74	0·41	4·90
Totals . .	1289	239	1528	13·38	14·07	13·49	100	100	100
Zymotic diseases .	55	11	66	·57	·64	·58	4·2	4·6	4·3

1. *Brain and nervous system.*—Of the whole number of deaths, 139, or 9 per cent., took place through this channel; and the particular diseases are indicated in the subjoined table.

TABLE IV.—*Showing the distribution of* 139 *deaths due to diseases of brain and nervous system.*

Disease.	M.	F.	Total.
Apoplexy	40	9	49
Softening of brain . . .	11	5	16
Inflammation of ditto . .	11	3	14
Abscess of ditto . . .	5	0	5
Tumour of ditto . . .	3	0	3
Diseases of ditto (not specified) .	17	2	19
Paralysis	13	4	17
Epilepsy	6	2	8
Diseases of spinal cord . .	7	1	8
Total	113	26	139

Of the 40 men who died of *apoplexy*, 32 were invalids or of a weakly constitution, and the age (of 26 recorded) averaged 40 years, the extremes being 18 and 76. The form in which it occurred is put down as 'serous' in 21, 'sangnineous' in 9, and in 10 it is not stated.

We are not told of any lesions in the brain, or elsewhere, that gave rise to the effusion of serum in the cases of simple (serous) apoplexy, but it is worth bearing in mind that the large proportion occurred in individuals of a debilitated constitution indicating pre-existing disease. Hence the term serous apoplexy, although it indicates the immediate cause of death, does not tell us upon what pathological condition the effusion and the death depended. The term is now in great measure discarded on this account, and its occasional use may be attributed more to its convenience than its correctness.

Dr. Wilks has pointed out the frequency with which this condition is met with in individuals suffering from Bright's disease, and I think investigation would prove a relationship between the two among prisoners.

In ten out of eleven cases of 'sanguineous' apoplexy, the cerebral hæmorrhage took place in individuals whose state of health was defective. The position of the clot as given in 5 cases was as follows: lateral ventricles two, base of brain, right hemisphere, and cerebellum one each.

With regard to the other forms of brain disease which proved fatal, the reports furnish little of a definite nature. But notwithstanding this it is impossible for us to forget the great importance that must be attached to cerebral pathology among criminals. From a medico-legal point of view the brain of the criminal as the organ of mind demands full consideration.

The antagonistic opinions which medical witnesses of high standing are found to maintain in our courts of law attest to the fact that the distinction between acts entirely criminal and acts merely insane is anything but well defined or understood. Besides this the prison surgeon in his daily work is constantly called upon to form an opinion as to the mental capacity of individual prisoners, and to attach a certain value to acts done by them of a mischievous or eccentric character. I have to mention here the fact that *insane* prisoners are removed from the convict prisons and are not included in the returns given.

Hence a certain portion of the actual mortality is lost sight of. I am indebted to Dr. Orange, the superintendent of the State Asylum at Broadmoor, for a return which shows that of 173 criminals (both sexes) received there from convict prisons, since it was opened in 1863, 23 died. This number of deaths it must be remembered occurs in the worst cases, for convicts who are simply weak-minded or imbecile—as well as the epileptics—are retained in prisons. Now, during the 15 years, 432 convicts (male and female) have been sent from government prisons to lunatic asylums as insane; being on an average 28·8 per annum. Calculating the whole mortality as being at the rate as at Broadmoor, it will be represented by 57 deaths; and this reckoned upon the whole convict population gives an average annual rate of ·5 deaths through insansity per 1000.

2. *Respiratory system.*—The lungs undoubtedly are the most vulnerable of organs, and in none more so than among prisoners. Diseases of the respiratory organs cause more than 50 per cent. of the mortality among them; and even if we exclude phthisis, the lungs show a high per centage of fatal diseases in the form of *pneumonia* and *bronchitis;* although there can be no doubt some of the deaths attributed to these were hurried on by the appearance of phthisical symptoms. Pneumonia and bronchitis contributed 65 and 68 deaths respectively; and in connection with these is best evidenced one of the great features of inflammatory diseases among prisoners; I mean the absence of acuteness. They seem to come on more insidiously than among people outside, and to be *grafted* upon some previous disease or upon a general debility. Nothing struck me more in my work among the 'casual' or every day sick among nearly 1600 prisoners employed on public works in the variable climate of Portland than the active and pronounced physical signs which an examination of the chest revealed in men whose pulse and general aspect were comparatively calm and free from fever; and I felt the necessity of having constant recourse to the stethoscope as a safeguard in dealing with them.

Not only is this obscurity of approach observable; but on the other hand, inflammations of the sthenic type—with flushed face, and pulse bounding and suggestive of depletion—are remarkable for

their absence; and this too in men selected as able-bodied and fit for hard labour.

Of the deaths due to other pulmonary affections 15 are put down to pleurisy, 14 to hæmoptysis, 4 to empyema, 2 to abscess, and 1 each to gangrene and hydatid.

Phthisis.—And now we come to treat of that disease which seems to mow down with unswerving relentlessness the prison population, pulmonary consumption. Well may it be termed 'the scourge of our prisons,' or rather, I should say, of the criminal classes, for I believe prison is more the reservoir than the source of consumption. I do not deny that imprisonment may contain in itself noxious springs in the form of depressing influences; but it contains hygienic springs too, in the form of regularity, sobriety, and cleanliness. Nearly one half (41·29 per cent.) of the deaths among prisoners is ascribed to phthisis. But this does not represent the full extent of its ravages; for it is to be borne in mind that in some affections, as in tubercular peritonitis, phthisis is really the prime mover; while in other cases it appears to be the complementary agent whereby some other diseases not necessarily fatal are enabled to carry off their victim. We shall direct our attention to phthisis as the immediate cause of death, and although we shall confine ourselves as far as possible to general points, the interesting nature of inquiries into this subject will readily be seen.

During the 15 years 631 prisoners died of phthisis, 527 men, and 104 women. The annual rate per 1000 is 5·57; the number being 6·12 among the women and 5·47 among the men. There were nearly 3 per cent. more deaths from this cause in the female population than in the male; the mean rate for all prisoners being 41·29 per cent. of the whole mortality.

The average age of 411 men who died of phthisis was thirty-three years five months, and of 68 women twenty-seven years nine months. From the circumstance that a prisoner when he becomes phthisical or the subject of prolonged debility or disease is sent from public works to a light labour or invalid station, it is a difficult matter to show the number of deaths from consumption that took place among the healthy men retained in this country. The following figures will help us to form some idea on this point. The healthier class of prisoners are sent to public works, prisons, and we find that 95 out of the 527 deaths took place there. This indicates the number of more acute cases of phthisis that proved fatal among this class. If to this we add 65 deaths that occurred at Dartmoor and Woking, of men invalided from public works, we get a total of 157 who died of consumption, and who since imprisonment had been more or less able-bodied. I dare say the number should be increased by the addition of a few more whom I am unable to trace, they having been invalided to one prison and then passed on to another;

but, on the other hand, it must be considered that even of those received at public works a proportion were only in 'moderate' health.

The amount of invaliding due to consumption is not within the range of this paper; but I may quote with advantage the following remarks from the report of Dr. Campbell, the medical officer of Woking Invalid Prison, for the year 1867:—"Out of 17 deaths from phthisis during the past year, 12 had been invalided for it, the ground of invaliding in the remainder having been bronchitis, infirm and aged, debility, and deformed. They were all greatly reduced at the time of reception, 12 continued under treatment in hospital till death, and the others were only capable of a little nominal work when discharged to the prison, as they sometimes were with a view in most cases to gratify their wish for a little change. These men in general acknowledge to have been long subject to cough or other chest affections, or to hereditary predisposition. In the less advanced form of the malady there was often a very satisfactory improvement, and some have become as robust and healthy as to justify me in recommending their removal to public works; no doubt some of them may have relapsed, but as every possible precaution is taken before sending them, I hope most have done well. Many others were retained here, liability to colds or exposure unfitting them for removal, but these men were often capable of performing a fair amount of work during the intervals between their attacks, and generally showed a desire for some useful employment."

The *pathological* relations of phthisis among prisoners are highly interesting and important. I mentioned a little ago how insidiously inflammatory affections of the chest among them crept upon our notice, and what unexpected physical signs frequently revealed themselves on our first examination with the stethoscope. Cases of the sort are of no infrequent occurrence on public works and among prisoners who by repeated misconduct bring dietary and other punishments upon themselves. In such, pulmonary disease has all the appearance, as I said, of being *grafted* and nourished upon some morbid condition, local or general, that has already taken root and established itself. Phthisis is very apt to spring from this combination, and every prison surgeon knows how rapidly the victim often sinks when the disease takes on this phase.

A low asthenic form of pneumonia attacks previously weakened longs, and ushers in the night sweats, the emaciation, the fever, and increased temperature of phthisis, and when death takes place, indications of advanced and long standing disease are found at the post-mortem examination. These pathological appearances often prove a greater puzzle, and, taken on the old doctrine of Laennec, which maintained the presence of tubercle in all cases of phthisis, simply unexplainable. The same appearances, on the other hand, form

according to the more recent pathology of phthisis, a rational exposition of the clinical history of the cases.

So important is this later pathology, and so essentially is consumption a disease of prisoners, that we may be at liberty to look briefly into the subject. Considerable modifications have taken place of late years in the opinion of our own authorities as to the relation between pulmonary consumption and tubercle; but German pathologists especially have turned the attention of workers in this direction; and their doctrines have found an able exponent in Professor Niemeyer of Tübingen, in whose 'Text Book of Practical Medicine' and 'Clinical Lectures on Pulmonary Consumption' (published by the Sydenham Society), they are clearly set forth. The great principle insisted upon is that *phthisis* may exist *without tubercle*. Niemeyer holds that the majority of cases of consumption are the result of pneumonia or repeated pneumonic processes terminating in cheesy infiltration and disintegration of the lungs.

It is acknowledged that this caseous infiltration may lead on to, or be succeeded by the development of tubercle as a secondary occurrence, and indeed Niemeyer "has no hesitation in saying that the greatest danger to consumptives is that *they are apt to become tuberculous;*" an event which mostly happens towards the fatal termination of the case. As to tubercle, he recognises but one form —the miliary; and this he says is rarely found in a being which does not contain products of chronic inflammation. Pulmonary consumption is referable, therefore, either to caseous infiltration (the result of pneumonia—mostly of the chronic catarrhal form) or to chronic tuberculosis, wherein tubercles make their appearance. Besides there are cases of acute miliary tuberculosis which appear in the form of acute febrile disease more akin to typhus.

The doctrines, of which I have given only the outline, appear to me to throw much light upon the causation and pathology of phthisis among prisoners; and I believe a study of their application to the history of that disease, in the criminal class, will go far to confirm them as accurate and truthful.

Indeed, so long ago as 1857 and 1858, Mr. L. Bradley, then medical officer of Pentonville, seemed to recognise the relationship when he returned as the cause of death in two cases "inflammation of lungs terminating in phthisis," and "phthisis following an attack of pneumonia." And I may say that in prison nothing is more easy than to trace many cases of pneumonia into phthisis; and nothing more difficult than to dissociate phthisis from pre-existing pneumonia or pulmonary affection of an inflammatory nature.

The establishment of these newer doctrines would often encourage us to form "less gloomy prognosis in consumption, and inspire us with fresh hopes in its treatment."

The improvement, even to a more or less lasting recovery, which

takes place in many of the 'phthisis' cases invalided to this establishment from other prisons, is wonderful.

I do not mean in what I have said on this subject to deny the occurrence of tubercular phthisis in a *primary* form with evident miliary deposit.

In his 'Lectures,' the following propositions are accepted as conclusions by Niemeyer:

1. The consolidations and destructions of the lung, which form the anatomical basis of pulmonary phthisis, are, as a rule, the products of pneumonic processes.

2. Pneumonia, resulting in cheesy infiltration, occurs chiefly in delicate, badly nourished persons.

With reference to these we saw, in the early part of the paper, that prisoners, physically speaking, belong to and are an *ill-conditioned* class, and we have found that nearly one half of the mortality among them is caused by phthisis. But having got so far, we have to admit that it is not so clear that the consolidations and destructions of the lung, which form the anatomical basis of the phthisis, so occurring (*i. e.* very frequently, and among prisoners, an unhealthy class) are, as a rule, the products (cheesy infiltrations) of pneumonic processes. But in evidence of the correctness of this proposition, I venture on leaving the subject for the present, to submit the following points:

1. That the anatomical basis of the phthisis, occurring among prisoners, is often a pulmonary solidification, due to cheesy infiltration, unaccompanied by tubercle.

2. That such consolidations are found in prisoners dying of phthisis, in whom the signs and symptoms on admission were referable only to inflammation of the lungs.

3. That limited consolidations of the same character, as well as pleuritic adhesions, and other pneumonic indications, are frequently found after death in prisoners who were not suspected of being phthisical, and who did not die of consumption.

4. That in prisoners who die of consumption, other organs, more especially the kidneys, are found to be the seat of degenerate cell-formation, for the most part non-tubercle in character.

5. That many prisoners, invalided for phthisis, require admission to hospital, from time to time, for slight catarrhal attacks, indicating a tendency to the occurrence of inflammatory processes in the lungs.

6. That the comparative frequency of apparent recovery and arrestment of the disease, among prisoners invalided for phthisis, betokens a more recoverable state of the pulmonary organs than we are justified in expecting where tuberculosis has established itself.

3. *Circulatory system.* Diseases coming under this head proved fatal in 130 cases; of which 107 were in males and 23 in females.

Heart disease caused 104 deaths; aneurism, 15; pericarditis, 8; rupture of large vessels, 3.

Heart disease occupies a considerable place among the maladies of prisoners, and if we remember the irregularities and the alterations of excitement and depression to which their previous mode of life submits them we shall be the less surprised at this. Of the 104 deaths from this cause, 88 were in men and 16 in women. This gives us an average animal mortality of ·91 from heart disease per 1000 of prison population; the corresponding number among males and females being ·91 and ·94 respectively. If we divide the fifteen years into three quinquennial periods we find the average mortality for each period to be as follows:

	1st per.	2nd per.	3rd per.
Males . .	·64	·93	1·15
Females . .	·57	·48	1·81

This table shows a steady increase in the numbers among males up to nearly double; while among the females it shows a remarkably sudden rise in the third period to more than three times the rate of the first period.

This increase among the males tallies with the statement made by Dr. Quain, in his Lumleian Lectures[1] this year, which showed that in adult males the proportion of deaths from heart disease is greatly on the increase. On the other hand, however, he informs us, that "the same statistics show there is almost no rise in the per-centage of deaths of females from cardiac disease during the twenty-five years, from 20 to 45." The only explanation of these different results that I am at present able to offer is that, granted a corresponding liability to heart disease, or to an increase in the number of cases of heart disease in both sexes, it will follow that women, from their weaker constitutions, are less able to resist the influence of the disease; and it will be at once acknowledged, I think, that women of the criminal class are more on a footing with men of the criminal class, as regards liability to disease, than are women in the general population with men in the same. A reference to Table III will show that, with one exception, the per-centage of deaths due to the various orders of disease was in every case higher among the women than among the men. The exception is with regard to 'other diseases' (*i. e.* all, excluding phthisis) of the respiratory system, wherein the males show a considerably higher proportion, owing, no doubt, to the greater atmospheric vicissitudes to which they are exposed out of doors and on public works. Hence it is reasonable to conclude, as far as the testimony of the death-rate goes, that female prisoners are as a body more unhealthy and delicate than male prisoners. No reliable information is to be gleaned

[1] 'Lancet,' March 23rd, 1872.

from the reports as to the form of cardiac disease which conducted to death. It would seem that valvular disease and hypertrophy are usually present and in association with each other; while in a considerable number the heart is returned as "fatty." My own impression is, that atheroma and the calcareous transmutation in the first portion of the aorta most frequently present themselves, and lead on to hypertrophy and dilatation. The heaviest heart I have met with weighed 34 ounces, and there the beginning of the aorta was completely encircled by calcareous deposits.

Aneurism.—Of the 15 deaths from this cause, 10 were in males and 5 in females, certainly a high relative proportion in the latter. In 3 of the cases the situation is not given; in all the others the aneurism was aortic: thoracic in 8, abdominal in 2, combined thoracic and abdominal in 1, and not stated in 1. The 'unfortunate' circumstances of the class from which our female prisoners especially come readily suggests syphilis as the probable cause of aneurism occurring among prisoners, and may explain the relatively high proportion among the women. On this subject Dr. Rendle, of the (then) female prison, in his Report for 1864, thus remarks:—"This disease (aneurism) occurs not unfrequently among female prisoners, and is, I believe, induced by a change in the coats of the large blood-vessels, which is of a syphilitic origin."

Pericarditis is recorded as the cause of death in 7 males and 1 female.

4. *Digestive System.*—In the eye of the prisoner no system deserves more (and, he will say, receives less) attention than this. Its working capabilities, he feels, are in excess of the encouragement afforded to it in the way of food-supply; but so, also, says the *honest* poor man. However, as a pathological subject in prisoners, its interest is secondary.

Reckoned upon the whole, mortality in both sexes, 8·7 per cent. of the deaths are due to diseases of the digestive organs, the total number being 133 (males 108, females 25). Of these deaths, 52 were due to peritonitis and enteritis, 35 to dysentery and diarrhœa (one or two being of a choleraic nature), 27 to diseases of the liver, 10 to hernia, and 9 were of an anomalous nature. Undoubtedly a proportion of the cases where the peritoneum was involved had a tubercular origin, but the list includes one death each from rupture of the stomach, colon, and gall bladder.

Dysentery and diarrhœa will be referred to under 'zymotic diseases.'

Hepatic disease is not prominently fatal among prisoners, but even in cases where death is caused otherwise, post-mortem examination frequently reveals extensive alterations in the appearance and structure of the liver. The 'nutmeg' liver, due either to amyloid or fatty degeneration, is the condition most frequently met with;

and, where the organ is altered in size, enlargement seems to be the almost invariable condition. The advanced state of cirrhosis with contraction and atrophy is rare, when we consider the numbers of wretched chronic drunkards that come under treatment in prison. The circumstances of prison life, involving a regular, though restricted, diet, and abstinence from spirituous liquors, tend to correct rather than to create diseased states of the liver, and we cannot, therefore, attribute to imprisonment the frequent occurrence of pathological changes in its structure. That frequency seems to depend, first, upon the great prevalence of consumption among prisoners, and, secondly, upon habits of dram drinking indulged in prior to conviction. As far as actual deaths are concerned, liver disease shows favorably in females as compared with males, the numbers being 2 among the former, and 25 among the latter. The relative mortality is striking, and I am inclined to refer the difference to the presence among the men of some old soldiers, whose residence abroad has helped to develop the disease in a more active form.

The 10 deaths from *hernia* were, with one exception, among the men, and are referable, most likely, to their labour. We are not told whether they were operated on or not.

5. *Urinary system.*—Diseases of the urinary organs caused 55 deaths (10 of them being in females). This gives a per-centage of 3·6 upon the whole mortality, a rate which I am sure fails to represent the influence which renal diseases exert among the causes of death.

From a pathological point of view, *degeneration of the kidneys* must be looked upon as of the first importance. Whether it presents itself (secondarily) as a morbid condition more frequently than the liver I am not prepared to say, but the aggregate mischief caused by it certainly seems to be greater.

As the *origo mali* in many dropsical effusions, and even in some cases of heart affection, renal disease must not be overlooked, while secondarily it is a common associate of phthisis and other constitutional diseases of a low type. The enlarged fatty kidney, and the somewhat smaller tough kidney of amyloid degeneration, are met with; the former more frequently, I think, than the latter. Cysts are of no infrequent occurrence.

The *bladder* sometimes leads on to death by chronic inflammation or through malignant disease.

6. *Constitutional and general diseases.*—The variety of diseases that come under this heading makes useless for us to attempt any general comparative statement with regard to them. The nature of this 'various' group will readily be understood by reference to the following table, which sets forth the numbers in both sexes, as well as the total mortality due to the individual causes.

TABLE V.—*Showing mortality from constitutional and general diseases.*

Disease.	Males.	Females.	Total.
Strumous diseases, including lumbar and psoas abscess .	45	8	53
Cancer	19	10	29
Dropsy	13	5	18
Fevers	17	2	19
Erysipelas	10	—	10
Diseased bones and joints .	16	1	17
Debility	12	2	14
Age and decay . . .	7	—	7
Diabetes	5	1	6
Anæmia and syncope . .	5	—	5
Pyæmia	4	—	4
Syphilis	1	1	2
Rheumatism . . .	2	—	2
Uterine disease . . .	—	2	2
Ulcer	2	—	2
Purpura	1	—	1
Tumour	1	—	1
Atrophy	1	—	1
Total . . .	161	32	193

Struma or *scrofula* can scarcely be called a *fatal*, although a very common, disease among prisoners. Its great frequency among them is rather to be recognised as a constitutional state or cachexia, inherited or acquired, which renders them at once more liable to the accession of diseases, and less able to resist their influence.

Its presence is indicated by a general unhealthiness of aspect, with or without glandular swellings, skin eruptions, or other external manifestations.

Struma goes hand-in-hand with phthisis, and through this channel it acquires its largest connection with the death rate. But when it chooses to act more directly, it presents itself in the form of large abscesses, which often work sad havoc before they extinguish life. Apart from the deaths from diseases of bones and joints (17 in number), the majority of which might be included in the same category, diseases of the strumous class were fatal in 53 cases (45 in males and 8 in females). Of these, 39 deaths were from abscess, the great majority of which come under under the designation 'psoas' or 'lumbar.'

Cancer was fatal in 29 cases: 19 males and 10 females. The seat in order of frequency is given in the following list:

Stomach	9
Liver	5
Uterus	4
Lungs	3
Intestines	2
Breast	1
Urinary Bladder	1
Bones of Spine	1
Not stated	3
	29

The particular form is seldom given. *Diabetes* caused 6 deaths, one of them being in a female.

Veteran criminals to the number of 7 became extinct through *age and decay*, but a number of the old men are carried off by pulmonary and other attacks.

Zymotic diseases.—We now come to consider a class of diseases, the zymotic, which are of peculiar interest to us. We shall first dispose of the enthetic, dietetic, and parasitic orders, by stating that only two deaths (syphilis) were due to the first, one (purpura) to the second, and none to the third.

The *miasmatic* order (to which we shall refer in our use of the term zymotic disease) claims 66 deaths for the whole 15 years; males, 55; females, 11.

The total number of deaths represents an average annual mortality from zymotic diseases of 4·4 in a mean population of 7551 prisoners, or of ·58 per 1000.

Zymotic diseases cause 4·3 per cent. of the deaths from all causes.

Table VI shows the fatal diseases belonging to this class.

TABLE VI.—*Mortality from zymotic diseases.*

	Disease.	Mortality for Fifteen Years.			Mortality, Quinquennial Periods.		
		M.	F.	Total.	I.	II.	III.
	Diarrhœa and dysentery	26	9	35	8	19	8
Fevers.	Typhus	5	1	6	...	4	2
Fevers.	Typhoid	5	1	6	3	2	1
Fevers.	Unnamed	5	...	5	...	3	2
	Smallpox	1	...	1	...	...	1
	Erysipelas	10	...	10	4	3	3
	Rheumatism	3	...	3	3	—	—
	Totals	55	11	66	18	31	17

Diarrhœa and dysentery.—The latter disease does not often present itself in prison in its true form. Diarrhœa, resulting for the most part from atmospheric influences, but sometimes from dietary changes also, not unfrequently makes its appearance as a passing epidemic. The rarity of death shows that it is not usually severe, but the early application of prisoners when attacked, and the means adopted to check when possible, any general cause of the disease, no doubt exert a favorable influence. Diarrhœa and dysentery together are the main cause of death in the zymotic class, being fatal in 35 out of the whole 66 cases.

The few deaths from infectious and contagious diseases also speak very favorably. It is not to be expected that prisons should be *absolutely* free of such; but while a *few* deaths indicate their presence, they show also that some influence is at work, or some means adopted whereby they are prevented from spreading. Fevers, smallpox, and erysipelas, caused only 28 deaths in the fifteen years in the whole population, and only one death in each of the three years last reported upon.

These results are the more interesting when we remember the large amount of disease, as well as the general low tone of health, which characterise the inhabitants of our prisons. The separation of convicts from the general public (except through the officers), and the means at disposal for complete isolation when individuals are seized with contagious disorders, as well as the habits of cleanliness and regularity enforced throughout the prisons, must exert a powerful and favorable influence in preventing the approach and the spread of zymotic diseases. The importance which diseases of this class have in relation to the question of public health has induced me to make some pointed allusion to them. Exactly a century ago the English prisons could only be looked upon as a very hotbed of pestilence and squalor. The tide had risen high, but its height had been reached, and the sanitary act of 1774 was, thanks to the indefatigable labours of Howard, the signal for its ebb. The hundred years have not been without their work; but that work has proved a triumph, and I believe sanitary science can point to no more signal success than the facts that for the past 15 years, ending with 1870, the convict prisons of England have been entirely free from fatal epidemics, and that the mortality in them from zymotic diseases has been reduced to a minimum.

They now take rank, as Dr. Guy has said, "among the healthiest abodes of men."

7. *Accidents and Violence.*

The violent deaths were altogether 74 in number, being at the rate of 5 per annum for the 15 years.

They are thus distributed:

Accident	49
Suicide	24
Homicide	1
	74

Only one violent death occurred among the females, and that was a suicide.[1]

Accidents.—The accidental deaths took place, as might be expected, for the most part while the prisoners were engaged at the heavier sorts of work. The prisons at which fatal accidents have occurred, and the number at each, are given under:

Public Works	Portland	23	39
	Chatham	9	
	Portsmouth	7	
	Millbank	4	
	Pentonville	3	
	Dartmoor	2	
	Woking	2	
		50	

Of course the large proportion occurred at public work prisons, and it would not be surprising if fatal accidents were more frequent, when we think how many of the prisoners have been unused to hard labour, or else are lazy and careless. Portland itself contributes nearly the half, but this is most likely due to the greater variety and uncertainty of the work there, in quarrying and excavating.

In some of the cases death took place after primary amputation of limbs; and in more than one case death resulted from injuries self-inflicted (as by putting the foot under waggon wheels) with a view to evade labour.

One man at Pentonville while at work in the kitchen stole a piece of meat. In swallowing it, it stuck in his throat and he was suffocated, he himself in trying to extract it having forced it into the larynx.

Another, at Millbank, trying experiments with belladonna ointment, poisoned himself unintentionally. Having received the ointment for the relief of his pain, he appears to have kept it and afterwards to have swallowed some with the view of producing illness.

At the same prison one prisoner killed his fellow by a blow.

The following case—a warning to "smashers"—may be given here. I copy it from the report of Mr. Bradley, late Medical Officer for Pentonville, for 1868: "Convict J. P. was suddenly seized with

[1] 73 violent deaths among the men, give ·7 per 1000 per annum; and if we deduct this from 13·3, we get 12·6 as the average annual mortality *due to disease* among the males.

vomiting of blood, and died of hæmorrhage a few hours afterwards. The post-mortem examination discovered a counterfeit half-crown lodged in a pouch in the gullet, which had caused ulceration and perforation of the aorta. The prisoner had been a 'smasher' and in order to escape detection swallowed the coin referred to about ten or eleven months before his death. The case is remarkable for the absence of any difficulty in swallowing food or other symptom indicative of the presence of a foreign body in the gullet."

Suicide.—In the fifteen years, 24 convicts committed self-destruction, being at the annual rate of ·21 per 1000 of the whole prison population. Only one female destroyed herself.

The mode adopted was as follows:

Hanging (1 female) . . .	16
Cut-throat	4
Precipitation from a height . .	2
Not stated	2
	24

The prisons at which they occurred:

Pentonville	11
Millbank (1 female) . . .	10
Portland	2
Chatham	1
	24

Curiously enough the two cases at Portland occurred within a fortnight of each other.

When we consider the forlornness of prison life, the rate of suicide cannot be reckoned a high one. The three chief features of suicide among prisoners are, first, that it is usually committed during the earlier months of imprisonment (*i. e.* when the feelings of remorse and disappointment may be supposed to be keenest); secondly, that in the majority of cases no mental derangement had manifested itself prior to the act; and, thirdly, that it is of rare occurrence among female prisoners.

V. *Has the mortality of the Government prisons of England increased or decreased?*

This question may be taken either in a *general* sense, applying it to the whole circumstances of the case, or in a *special* sense, wherein reference is made only to the mortality *as due to imprisonment.* With regard to the special or limited question, I think we may with tolerable safety, especially after what has been said about zymotic diseases, reply that the mortality has decreased. But we shall see that the large question as to the increase or decrease is not so easily answered. If we are content with bare numbers, a reference to the

tables will tell us that the death rate has both increased and decreased within a given period; and this suggests a difficulty as to what constitutes an increase or decrease. Now if we are prepared to general conclusions, as regards mortality say, simply because we find a rise or fall of 1 per 1000 upon a population of 7000, we must be prepared at the same time to show that the circumstances affecting that mortality have been uniform, or, if not uniform, equally balanced *pro* and *con*.

The circumstances of our convicts as bearing upon the question of mortality have not been uniform; and passing over for the present the fluctuating annual rate of death, our investigation must begin by inquiring what modifications have taken place, and how far and in what direction they have manifested their influence.

Those influential modifications are referable to one or other of the following topics relating to prisoners.

1. The physical condition on reception.
2. The dietary.
3. Pardons on medical grounds.
4. Transportation.

It will be more convenient as a matter of arrangement to begin with the two last, and for reference I have constructed the following table.

TABLE VII.—*Showing numerical distribution in quinquennial periods of deaths from all causes; deaths from phthisis; and of medical pardons and transportations.*

	1st Period, 1856—1860.		2nd Period, 1861—1865.		3rd Period, 1866—1870.	
	Total number.	Average annual number per 1000 of pop.	Total number.	Average annual number per 1000 of pop.	Total number.	Average annual number per 1000 of pop.
Deaths from all causes .	483	12·8	494	13·6	551	14·0
„ „ phthisis .	210	5·5	211	5·8	210	5·3
Medical pardons . .	82[1]	2·1	28	·7	12	·3
Transportations[2] . .	3849	102	3706	101	1658	44

Medical pardons.—The value of pardons on medical grounds, as an item in the bill of mortality depends, of course, on the probabilities which the individuals would have had of dying during their imprisonment. Now, in 1857, the Secretary of State drew attention to the abuse of medical pardons which then existed, and said that those so pardoned frequently returned to prison with fresh sen-

[1] Deducting 14 cases pardoned as being of weak mind.
[2] Including those sent to Gibraltar.

tences. For instance, of 96 pardons in the first quinquenniad, 14 were granted because the prisoners were 'weak-minded,' and these cases may fairly be deducted as not being of fatal import. Of the remaining number, the majority were consumptives, some of whom got out that they might have a better chance of recovery than in prison. Sometimes the release was effected in order that an operation might be performed. In several others it was granted only a few months before the due time. In 9, 'general debility' formed the grounds for pardon. During the third quinquenniad greater stringency was exercised, and only 12½ medical pardons were granted. With an equal fatality in the cases, it is clear that the reduction of medical pardons goes towards increasing the mortality among prisoners; *i. e.*, the number of medical pardons bears an inverse ratio to the mortality. But the fatal import of the cases is not equal, and it is impossible, in the absence of positive information, to adjust precisely the comparative value of recent and former medical pardons. I am willing to proceed upon the following terms, viz. considering the great laxity during the first period, to accept one-third (the proportion given by Mr. Bruce Thomson and other authorities) of the medical pardons in that period as deaths; and, bearing in mind the stringency of more recent years, to reckon the 12 medical pardons in the third period as deaths. On the whole, in this relation, I think there is more (or, at least, as much) risk that we grant too many as deaths in the third period than that we accept too few as such in the first.

Transportation.—The alterations on this score are involved in its cessation during the later years of the convict system, and the extent of the change may be thus stated: that whereas 102 men per 1000 prisoners were sent abroad annually, in the first period only 44 were sent in the third. Now transportations and medical pardons of convicts exert antagonistic influences on the mortality. The great reduction in the number of both during later years implies, in the case of transportations, the detention in English prisons of so many convicts of the healthiest class, *i. e.*, of those *least* likely to die; and in the case of medical pardons, the detention of so many convicts of the most diseased class, *i. e.*, of those *most* likely to die. We must see, therefore, what relation the one case bears to the other. Beginning with the first period, if we take one third of the medical pardons as deaths we find that the relation of such pardons per 1000 of population to the transportations per 1000, to be as 1 to 145; and in the third period, if we accept the 12 medical pardons as deaths, the relationship is found to be exactly the same.[1]

So that whatever small differences may be suggested, on one side

[1] This result is so curious that I must state that I had fixed upon the terms as to medical pardons *before* I made the comparative calculation.

or the other, it may reasonably be accepted that in the matter of death-rate the opposing influences of medical pardons and transportations are balanced.

Dietary.—In the year 1864 the introduction of a reduced scale of diet for convicts was effected. We do not here intend going into this subject further than its relation to the mortality is concerned. Now I may safely assert that no subject has received more anxious attention from the authorities than that of the diet. While nothing is more to be deprecated, from a socio-criminal point of view, than any over-feeding of prisoners as such, nothing could be more unjust than an enforced starvation of them. A prison diet, speaking generally, ought to be the minimum diet which is compatible with a due performance of the healthy bodily functions, as well as of the work implied in penal servitude.

Diet does not affect mortality except through disease, and hence a full consideration of the question of the amount and the effects of particular diets ought to be undertaken in connection with the *diseases* of prisoners. But we may point out that the indirect effects upon prison mortality of insufficient diet would show themselves by causing an increased vulnerability as to disease; and any reduction of diet towards a minimum necessarily tends to the reduction of the disease-resisting powers of the mass, and herein lie the risks. But with regard to convicts, those risks are in a measure compensated for by the adaptation of employment to physical condition, and by the readiness with which they may be admitted to hospital, where a suitable diet, alterable at discretion, is authorised. On the whole, notwithstanding counteracting *provisos*, a reduction of diet cannot but exert a comparatively unfavorable influence on the mortality, in so far as it is the first in the field, and it is not until its effects show themselves that the remedies are applied. It may seem a more comfortable way of putting it if we say that an increase of diet would tend to reduce the amount of disease, and, it may be, of death, among our convicts; but the same argument is not so comforting when we think of the element of encouragement it would afford to garrotters, burglars, and other disturbers of our domestic peace.

Lastly, we have to revert to the *physical condition on reception.* We found in the beginning of this paper that prisoners were essentially an ill-conditioned class, and now I have to state that the unhealthiness of convict recruits has been on the increase. In his Report for 1867, Mr. Gover, of Millbank Reception Prison, alludes to the "fact that the proportion of convicts who are in a diseased or infirm state on their reception is on the increase." And he gives a table which shows that the average daily number of infirmary sick in that prison increased steadily from 1859 up to 1867, when it had become exactly double. The directors themselves refer, in their

Report, published in 1868, to the "great increase in recent years in the proportion of convicts of a weakly and diseased constitution." Accepting this as of course unfavorable to the decrease of convict mortality, I now proceed to my argument.

The changes involved in the four topics with which we have been dealing have come into operation during the more recent years of the convict system; consequently, the more recent years, taken with relation to our subject, fairly constitute a *period* in that system, and therefore the mortality of the five years last reported upon form a legitimate basis for comparison with previous periods of five years. Now, the whole fifteen years embraced in our inquiry have, except in respect of these four topics, been remarkably uniform as to influences bearing on the death rate. There has been no fatal epidemic to complicate, nor has there been any special cause of unusual mortality; so that, regarding the middle period of five years in the light of a transition period, we go on to compare the third period ending with 1870 with the first period ending with 1860.

And first as to figures, Table VII shows that the average annual number of deaths per 1000 of population from all causes rose from 12·8 in the first period to 13·6 in the second, and to 14·0 in the third. We thus arrive at an increase of 1·2 in the annual mortality per 1000 in the third as compared with the first period. And what have we found to be the direction of the influences exerted by the four sets of modifications which characterise, be it remembered, the third period? The only influence out of the four which favoured the decrease of the mortality was the cessation of transportation, and that influence we found to be exactly neutralised by the reduction of medical pardons. We are thus left with a reduced dietary and an increased ratio of ill health among convict recruits, both of which exert an unfavorable influence on the death rate; and it may be said, I think fairly too, that the *amount* of that unfavorable influence is represented by the increase of 1·2 in the average annual mortality per 1000 in the third period as compared with the first. But, not to put too fine a point upon it, I think that, having shown that there has been a progressive increase in the quinquennial rate of mortality, and also that the weight of characteristic influences favours that increase, I may claim to have proved at least that, upon the general question, the tendency of the mortality of the English convict prisons has been towards an increase. This leads us on to inquire to what forms of disease that increase is due.

Not to the 'scourge of prisons,' consumption, for, notwithstanding the annual fluctuations of its death rate, exactly the same number died from that disease in the third as in the first period; but the larger population in the former reduced its rate slightly. See Table VII.

Besides consumption, only the 'various' list of constitutional and

general diseases shows a comparative decrease (indicating, probably, defective classification of causes during the first period). All the other, classified, causes of death show an increase. The amount of increase or decrease per 1000 in the third period, as compared with the first, is given below.

Diseases.	Increase.	Diseases.	Decrease.
Brain and nervous system	·5 per 1000	Phthisis . . .	·2 per 1000
Respiratory system (excluding phthisis) .	·3 „	Constitutional and general	·9 „
Circulatory . . .	·7 „		
Digestive . . .	·1 „		
Urinary . . .	·3 „		
Violence . . .	·4 „		
Total increase . .	2·3 „	Total decrease . .	1·1 „

If we deduct the decrease from the increase we get 1·2, the general increase in the third period upon the first.

On this subject I may refer to some statistics published by Dr. Rendle, of Brixton Prison, in the 'British Medical Journal' for April, 1871 (No. 536). I took exception to conclusions based upon insufficient and immaterial data, and a brief correspondence ensued between us (see 'Journal,' Nos. 538, 543, and 546). Throughout his reports Dr. Rendle frequently deals with the question of mortality among female prisoners. About ten years ago, Mr. Bruce Thomson, of Perth, published in the 'Edinburgh Medical Journal' some statistics on the mortality, &c., among the prisoners in the General Prison for Scotland.

I append a table, which gives a numerical comparison as to the average population and mortality of the convict prisons of England, Ireland, and Scotland, for the corresponding fifteen years, viz. 1856-70.

Table VIII.

	1st Quinquennial Period, 1856—1860.			2nd Quinquennial Period, 1861—1865.			3rd Quinquennial Period, 1866—1870.[1]			For whole 15 Years, 1856—1870.		
	Annual population.	Total deaths.	Annual deaths per 1000.	Annual population.	Total deaths.	Annual deaths per 1000.	Annual population.	Total deaths.	Annual deaths per 1000.	Annual population.	Total deaths.	Annual deaths per 1000.
England	7523	483	12·8	7270	494	13·6	7858	551	14·0	7551	1528	13·5
Ireland .	2114	162	15·3	1619	109	13·4	1359	70	10·2	1697	341	13·0
Scotland[1] (general prison)	608	61	20·0	684	38	11·0	735	55	14·9	675	154	15·3

[1] Includes prisoners not sentenced to transportation or penal servitude.

VI. *Concluding remarks.*—The convict question, more especially its practical portion, is in great measure a medical one. 'Penal servitude' may be said to be a large experiment set on foot by the State with the view of proving under certain conditions how much work a man is able to do, and how little food he can do it upon. Penal serfs or convicts, with this object among others, are for a time separated from the outer world and the test is applied. The adjustment of food and work to physical capability, or rather perhaps the adaptation of individual capacities to certain standards of food and work, is one of the chief duties of the medical department. The same department deals with the sanitary and hygienic questions relating to prisons and their occupants, as well as with the bodily and mental diseases of the latter. The results obtained from investigation, in these different directions ought not, in these days of state medicine and sanitary laws, to be lost, the more especially as they are favorable indications of what can be done under proper management and discipline. It is to be regretted, on various grounds, that no general annual report is made as to the condition of the convict medical department. The individual medical reports from prisons, varying greatly in their character, afford no indication of the state of health, &c., of the whole convict body. In spite of this drawback, I have endeavoured in this inquiry to present a fair estimate of the rate of death and its causes in our convict prisons, *as far as the evidence of the fifteen years last reported upon* goes; but I have avoided, for the sake of simplicity, statistical comparisons with other bodies of individuals.

The following are the general conclusions at which I have arrived:

I. That the average population was 7551, the ratio of females to males being as 3 to 17.

II. That the total deaths were 1528, the ratio of females to males being as 3 to 16.

III. That the annual rate of death averaged 13·5 per 1000 prisoners.

IV. That more than one half (52·6 per cent.) of the mortality is due to diseases of the respiratory organs, and that pulmonary consumption is by far the most fatal disease among prisoners, 5 out of every 12 deaths (41·3 per cent.) arising immediately from it.

V. That diseases of the brain and nervous system follow next in point of frequency, and then diseases of the heart.

VI. That 5 per cent. of the deaths are due to accident and suicide.

VII. That there has been no fatal epidemic in the convict prisons for the last fifteen years, and that their mortality from zymotic

diseases has been reduced to a minimum (·5 per 1000 prisoners per annum).

VIII. That, while the mortality *as due to imprisonment* may be said to have diminished, upon the examination of the whole question the tendency has been towards an increase in the rate of death, which increase appears mainly attributable to a higher ratio of disease existing among the convicts on reception.

II.—On the Action of certain Neurotics on the Cerebral Circulation.

By Patrick Nicol, M.D., Physician to the Bradford Infirmary, and Isaac Mossop, L.R.C.P.E., late Resident Physician, Royal Hospital for Sick Children, Edinburgh.

The uses of the ophthalmoscope, one of the most valuable additions to modern means of research, are not exhausted by its employment for the diagnosis of disease. As a means for observing the phenomena of the circulation in the fundus of the eye, under varying circumstances, this instrument may procure for us valuable results in determining the laws of action in the smaller vessels of the nervous centres.

The object of this paper and those that follow it, is to record some observations on the condition of the *fundus oculi*, and especially of the optic disc, under the action of certain neurotic drugs. The circulation of the blood in these parts is of great interest, because it is very closely connected with that in the brain, and conclusions may be drawn from the one to the other, with considerable confidence.

The cerebral central artery of the retina, which appears in the centre of the optic disc, is a branch of the ophthalmic artery, and that again of the internal carotid. The ophthalmic vein opens into the cavernous sinus, and its ramifications are subject to much the same conditions of blood-pressure as prevail within the skull. But it is the capillary supply of the optic disc that is the most interesting and important. This is as it were just a small portion of the general blood supply of the brain. The whole subject has been fully set forth in Dr. Clifford Allbutt's recent work.[1] It appears from the researches of Galezowski (confirmed by Dr. Allbutt), that various vessels proceeding from the *pia mater* and choroid plexus, are distributed to the optic tracts and nerves. When the *fundus oculi* is examined with the ophthalmoscope, a more or less rosy tint is generally seen in the disc, especially on the apparent outer side (indirect method of examination), and this is caused by capillary vessels from the branches just referred to, not by branches of the *arteria centralis*. Hence in observing the increase or diminution of this reddish tint, we are in a manner observing the changes in the vascularity of the brain itself. It is especially from this point of view that the following observations on the actions of neurotics are made.

The experiments reported in this paper were made on healthy individuals. Those which relate to the human eye were carried out on the authors themselves; those which relate to animals, on rabbits.

[1] The 'Use of the Ophthalmoscope in Diseases of the Nervous System and of the Kidneys.' 1871.

The method of observation in the case of the eyes of the authors was the following:—Each observer made drawings of the fundi of the other, until a thoroughly correct representation was arrived at, and a familiarity obtained with the general circulatory phenomena, and with the varying tints of the disc and retina. At the same time, during the observations themselves, every precaution was taken that the ordinary state of the fundus as regards vascularity should not be interfered with; for this purpose, the experiments were made at nearly the same period of the day, and that degree of precision was observed which is so necessary in dealing with a structure, where very slight causes may produce considerable change. Both the direct and the indirect methods of using the ophthalmoscope were carried out. On no occasion were the results obtained by the two methods found to be contradictory. The direct method, however, is that which gives the greatest satisfaction, since by its adoption the smaller vessels of the disc and retina can be more clearly defined.

Before any drug was taken, a careful examination of each fundus was made, and the appearances were compared with the correct drawing. After exhibition of the drug repeated examinations of the fundi were made at variable intervals, and the changes were carefully noted.

In the case of the rabbits employed, the direct method has been chiefly used. This shows with facility the fundus of the animal's eye. The appearances differ from those of the human eye, since no distinct boundary of the retina and disc can be seen. The vessels seem to run horizontally across the upper part of the field of vision, and have not the appearance of emanating from any distinct centre, but seem to perforate the retina at several places. The part of the field near the vessels is of a pale buff colour; in the more distant parts, there is an approach to the tint of the human retina. How far the capillary vascularity of the rabbit's eye represents that of the brain, we cannot at present tell, save in the way of post-mortem dissection; but the latter process has as yet revealed an agreement between the two organs, in respect of vascularity.

So far, six drugs have been employed, viz. hydrate of chloral, bromide of potassium, alcohol, quinine, ergot, and belladonna. The number of separate observations on these may appear small; but the method of conducting the experiments, each involving the taking of a dose of some powerful drug by one observer, precluded very frequent repetitions. It is satisfactory, therefore, that unequivocal results, so far as we can see, were in most cases obtained.

The observations on chloral were made on eight occasions, when doses varying from ten grains to a drachm were administered. Certain common results were obtained, which are as follows:—The disc after the dose was seen in the case of the authors, to have a trans-

parent appearance resembling white wax; in the animal experimented on, an increased whiteness was observed; these phenomena we thought due to diminished capillary vascularity. The retina we found always paler after, than before the administration of the drug. The central artery, and vein, and their branches, were darker in appearance, and seemed to stand out more prominently. The conjunctivæ were more congested after the effects of the drug were fully manifested, (this was not observed in the rabbit). The pupils were thought to be more sensitive to light. Besides these common facts, some exceptional phenomena were observed. A state of capillary congestion was found in the left disc (which was examined second) on two occasions; once it came on very soon after the light was thrown on the retina, on the other occasion it was observed from the first; the dose was in both cases a scruple. A general haziness of the fundus was noticed on one occasion in both eyes, an hour after a half-drachm dose, and when sleep had been produced. After the drachm dose all the general appearances were increased in intensity. Sleepiness was caused by the ten grain doses, and sleep by those from a scruple upwards. The feeling of muscular languor was so decided after each dose of chloral, as to call for special remark.

In the case of the rabbit experimented on, about a grain and a half were first swallowed, and seemed to have little effect on the animal. The pale area of the fundus appeared to be increased. After this, three successive doses of about two grains each were administered, the state of the retina remaining meanwhile much as before. These doses, which were given during a period of about two hours, failed to produce sleep, but on giving a further dose of about nine grains, the effect was to send the animal off into a very sound sleep indeed; in this state, no red retina could be seen anywhere. As the rabbit appeared to be dying, the heart beating with great rapidity and weakly, the breath being drawn at longer and longer intervals, the brain was exposed. A good deal of blood was unfortunately lost from a sinus in doing so, but, notwithstanding that, the state of anæmia in the brain was of so much more decided a character than in other organs, as to warrant the inference, that it had been specially affected. The brain was laid bare while the animal was alive, half an hour after sleep commenced.

To sum up, therefore, in the case of chloral, a tendency towards anæmia of the brain seemed to follow its administration.

In the case of bromide of potassium, observations were made on eight occasions, the doses varying from ten grains to a drachm. The results of the smaller doses, were not altogether uniform; a certain degree of congestion appeared, however, to be produced. With half drachm and drachm doses, and once with a scruple dose, the disc and retina were congested even in ten minutes after administration, and this state of congestion went on increasing, as long as

examinations were made. Even after the lapse of several hours, the increased redness was still manifest. The vascularity was found to be greater, as the dose was increased.

The exceptional phenomena were, in one case, a brighter redness of the vessels after ten grains. In another case, a hazy condition of the vessels was noted, after a scruple dose. The sedative effects of the bromide were marked, in the case of one observer; but they were very slight even with the drachm dose, in the case of the other; the congested appearance being still, however, present in the fundi.

It seems a justifiable inference, that bromide of potassium produces congestion of the brain.

In the case of alcohol, three observations were made; the doses were, two drachms and four drachms of rectified spirit, and two ounces of brandy. The results were uniform, and in accordance with the known action of alcohol, viz. paralysis of the vaso-motor nerves; congestion of the disc with appearance of small vessels not visible before, and congestion of the choroid and retina, were the main results. On one occasion, the veins had a clouded aspect. The dose of brandy was taken three quarters of an hour after the exhibition of a drachm of chloral. The effect of the spirit in counteracting the anæmic condition brought about by the chloral, was exceedingly manifest. In half an hour, the disc and retina were congested, instead of being very pale, as they were when the chloral sleep was going on. The general effect was not so much to remove sleepiness, as to substitute for it a sort of semi-intoxication.

In the case of quinine, observations were made on four occasions, when doses of five and ten grains were administered. The effects of ten grain doses, were noted in the following terms:—"In an hour and a half, vessels on the disc appear smaller, and the disc itself of a papery whiteness." "In an hour and a half, the disc and retina both very anæmic. Disc looks *dry*, with the vessels smaller than usual running over it." In two hours and a half, "both sides now decidedly paler than last time." The characteristic tingling in the ears, was felt with the ten-grain doses.

With five-grain doses, the effects on one observer, were similar to those already mentioned, though not so marked; with the other observer on one occasion, when the quinine created considerable disturbance of the stomach, a diminution of the vascularity was observed only to a slight extent, and about an hour and a half after exhibition of the dose; otherwise, the fundus remained, as it had been at the first, rather congested.

Ergot was under observation on three occasions, the doses being from half a drachm to a drachm of the liquid extract of the pharmacopœia. With drachm doses, the effects on the vascularity were in the direction of diminution, as with quinine. Thus, to quote from our notes taken at the time, "In one hour, R. disc decidedly

paler, with retina also of a paler tint. Left also paler. Almost complete sleep produced." Again, "In twenty minutes R. disc paler, on apparent inner side decidedly; on outer side also a little. L. eye altogether a little paler." "An hour after administration, retinæ and discs of both eyes decidedly pale, and this is especially observable in the R. eye." "In an hour and a half, retinæ not quite so pale. Discs the same as before. The subject of experiment very sleepy." The hours were sometimes rather late on these occasions, but we have no particular reason to think that this confused our results, neither of us being accustomed to retire early.

With half-drachm doses of ergot, the results were more equivocal, a transitory paleness appearing, however, to be produced in about an hour after administration.

Belladonna gave on the whole different results to what the authors expected. They supposed its tendency to have been towards contraction of the small vessels (see, *e. g.*, Handfield Jones, 'Functional Nervous Disorders,' Chap. LI, p. 816), but in those of the disc at least the effect seemed to be, their congestion. Observations were made on five occasions; grain and half grain doses being taken, and large doses administered to an animal. On the occasions when we experimented on ourselves the characteristic dryness of the throat was always felt, and to a painful extent with the grain doses. Dilatation of the pupils was decidedly produced by the larger doses. In the fundus of the eye, the appearances of congestion were observed on every occasion. Once, with a half-grain dose they appeared to pass off in an hour, and to be succeeded by paleness. Notes of the commoner results are such as the following:—"In three quarters of an hour after gr. ss Ext. Belladonnæ, R. disc certainly congested; centre of it corresponds in tint with surrounding retina. Left disc and retina also perceptibly altered, being still deeper in tint." "In an hour and a half, disc and retina still congested especially at inner side, outer, perhaps rather paler than before. Left continues congested, but is perhaps paler than at last observation."

The records from the larger doses agree with this so much that they need hardly be quoted at length. "A decided pinkiness of the R. disc, especially towards the centre, but also at other parts. It shades gradually into the retina," "R. disc congested, its centre of the same tint as the retina," are samples of the notes made in an hour and a half or two hours after the exhibition of grain doses. The tint of the retina, was not found to be affected to any particular extent, by the belladonna. The drug was given, it should be stated, in the form of extract.

In the case of the rabbit, a wonderful immunity from the effects of belladonna was found. Only after the exhibition of about a drachm of the tincture, did a little congestion of the fundi of the eyes ensue. After further exhibition of gr. vii. ss. of the extract, which

the animal appeared to enjoy rather than otherwise, dilatation of the pupil came on. The appearance of congestion remained the same. The animal seemed little the worse.

These observations are, so far, scanty, but may still possess some interest for therapeutists. In carrying them out we have not confused our results by using atropine to dilate the pupil; we availed ourselves of this application only while gaining an acquaintance with the appearances of the fundi, and though its effects remained to a slight extent during one or two observations, they could hardly have been sufficient to affect the results, especially as one eye was always free from atropine, even on these last-mentioned occasions.

The general results as regards the six drugs treated of here, may be said to be, that chloral, quinine and ergot diminish the capillary tint of the disc, while bromide of potassium, alcohol, and belladonna increase it.

Chronicle of Medical Science.

REPORT ON SCANDINAVIAN MEDICINE.

By J. W. Moore, M.D., M.Ch., Dubl.; Ex-Schol. Trin. Coll., Dubl.; Honorary Fellow of the Swedish Society of Physicians.

Twelve months ago, when engaged in compiling a notice of the recent literary labours of our medical brethren in Scandinavia, it was my happiness and my good fortune to have as my fellow-labourer, counsellor, and guide, one who has since been removed by the hand of death. My father, the late Dr. William Daniel Moore, even then prostrate in sickness, was yet able to dictate a considerable portion of that review. To have lost him must be my excuse for the inexperience and the shortcomings of the present article, of the defects of which I am fully conscious.

During the past year a number of communications, on subjects relating to practical, literary, and scientific medicine, fully equal to that of any former similar period, have appeared in the medical journals of Denmark, Sweden, and Norway. Most of these papers are of a very high order of excellence; indeed, all of them are above mediocrity. Within the necessarily narrow limits of a review like the present, it would be quite impossible to do full justice to the opinions advanced by the authors of these works and papers. Accordingly, I have concluded merely to give a brief analytical account of some, amongst many, contributions to the progress of medicine—contributions, which have appeared to me either to possess a certain degree of originality or to have been confirmative of researches already made in other countries.

The publication which naturally takes precedence in the medical literature of Scandinavia is one that represents all the nations included under this designation, namely, Sweden, Norway, Denmark, and Finland.

1. The present volume of the 'Northern Archives of Medicine,' the journal alluded to, and which, though in only the third year of its existence, has already won a European reputation, opens with an article by Professor W. Boeck, of Christiania, on leprosy in the United States of North America. The author reports the clinical history of eighteen cases of this affection. In nine instances the symptoms became developed in America, at longer or shorter intervals after the arrival of the patients in that country. These intervals varied from two years and a half to fourteen years, and the patients all lived in places where the disease had been quite unknown. The conclusion is obvious that leprosy may become

developed in America in persons who have gone to that country with a disposition to the disease, and that it may show itself even many years after their arrival; in the latter case, too, it may assume a very severe type. From Dr. Boeck's observations it would also appear that the disease, in the case of those already subject to it before they emigrated to America, generally becomes worse, while it is exceptional for it to remain stationary, or to show signs of improvement. Wherever any amelioration did take place, it was only such as might fairly be attributed to judicious treatment in hospital.

The frequency of hereditary or family transmission was well exemplified in these nine cases. Four of the patients inherited the disease directly; four others were members of families in the collateral lines of which leprosy had shown itself; and one only of the nine had no hereditary taint. Among the remaining nine patients, all of whom suffered from the disease while yet in Norway, five had had leprous relations, all in collateral lines; in the remaining four there was no suspicion of an hereditary disposition to the malady.

P. J. Wising, Medical Licentiate, of Stockholm, contributes some investigations on the anatomy and physiology of *Balantidium coli*, a parasite first described by Professor Malmsten in 1856. In 1869 a labourer, Lars Nilsson, 62 years of age, noticed for the first time the presence of small white worms in the motions from his bowels. About Christmas of that year he suddenly became affected with severe colicky pains, followed by considerable diarrhœa. This at last became so bad that there were as many as twelve stools a day; they were usually of the consistence of thin pea-soup, sometimes with firmer lumps intermixed, sometimes quite watery, and most frequently tolerably small in quantity. There was but slight tenesmus, while after some time the stools were streaked with blood. The microscope showed, in addition to the remains of food, a good many lymphoid cells, a few red blood-corpuscles, vibriones, and *Balantidium coli* in very large numbers. The treatment consisted in the occasional administration of an enema of oil and opium, and in the persistent exhibition of pills of carbolic acid. Enemata of carbolised water were also used from time to time. A comparative cure was effected after many months. But the more original researches of the author are directed to the question of the reproduction and propagation of the parasite. In a series of beautifully executed lithographs the anatomical structure of the animal, including the peristome, with its adoral ciliæ, nucleus, nucleolus, and contractile vacuoles, the phenomena of coition and the process of reproduction, are represented. The last-named is carried on in this way. The body of the parasite elongates so that its length becomes more than double its breadth, and the nucleus itself, also somewhat elongated, assumes a central position, with its long axis in the direction of that of the body. A tendency to constriction across the middle of both body and nucleus is the next change observed. This increases until the two halves are at last connected by a tiny filament only. The anterior half contains the old peristome quite unaltered, while

in the posterior half new adoral ciliæ begin to develope from the ordinary ciliary layer. At this stage the nucleus parts in two, so as to form a separate nucleus for each half of the body. Usually only one contractile vacuole is now found lying near the back portion of each half, for the anterior vacuole does not generally appear until the separation has been quite completed; both halves of the animal gradually take on a more independent life, and, finally, the connecting filament gives way, and the two halves fall apart as distinct living beings. The anterior half, which contains the original peristome, is a fully developed animal from the time of the separation; but the posterior half is as yet very unlike the full-grown parasite, into which it in time develops.

In 'Some Preliminary Remarks on Retinal Affections in Syphilis,' Dr. O. B. Bull, of Christiania, deals with a subject which is to-day attracting much attention. His investigations were made in about 200 cases of acquired syphilis, partly at the Christiania Chronic Infirmary, but chiefly, in the skin department of the Royal Hospital, with the assistance of Dr. Scheel.

The most usual objective phenomena presented by the retina in syphilitic patients are a moderate injection of the papilla of the optic nerve, congestion of the retinal veins, and a smoky darkening of those portions of the retina immediately around the optic disc. The injection of the last-named is a symptom which is found most frequently in recent cases, and which disappears after a tolerably short time. Hence its presence is, in some measure, a test of the date of the infection. Retinitis existed in *more than* 50 *per cent.* of the cases observed by Dr. Bull, while there was iritis in only twelve instances. On several occasions retinitis proved to be the first symptom of constitutional syphilitis; both eyes were generally affected, but in an unequal degree—more rarely one eye was quite healthy, or both were attacked with equal severity. As atrophy of the retina, with consequent amaurosis, is not a frequent *sequela* of syphilitic retinitis, the author regards an expectant treatment as especially indicated. He, however, has great confidence in an energetic exhibition of mercurials.

Under the very modest heading, 'A Contribution to our Knowledge of Empyema,' Dr. L. F. Toft, of Copenhagen, writes a monograph on this affection. As data for his remarks, he deals with thirty-seven instances of spontaneous primary empyema, which term he applies to those cases where from the first a pus exudation is the consequence of inflammatory action. In addition, 51 cases are referred to, in which the original purulent nature of the exudation could not with certainty be ascertained. In both classes the proportion of male subjects of the disease, was double that of women similarly affected. Of the 37 examples of primary empyema, 25 occurred in men and only 12 in women; of the 51 cases of empyema, in which absolute purulent origin was not a necessary factor, 36 occurred in men, and but 15 in women. The disease was most frequently met with between the ages of twenty and forty years. The effect of season was clearly shown by the statistics of all the

cases; the greatest number of patients being affected during the winter months, and again in April, when the changes of temperature were often excessive, and there was greater exposure from renewed work in the open air. As regards the *seat* of the effusion, it was *double* in 4 of the whole 88 cases, on the *right* side in 47, and on the left side in 37 instances, the respective per-centages being 4·6, 53·4, and 42·0.

It is to be regretted that space will not permit of a more detailed notice of Dr. Toft's important observations. He analyses the physical signs of empyema also. On one of the most peculiar of those accompanying pneumo-thorax, he remarks:—"*Metallic tinkling* appears to arise in two different ways, both of which are dependent on the presence of air in the pleural cavity. Either a bubble of air presses out from the opening into the lung, and bursts in the pleura, or a drop falls from its roof down into the fluid below. The second mode requires more room, and less fluid than the first; the bursting of an air-bubble is caused by, or follows, a respiratory effort, while the fall of a drop depends on the movements of the body."

'Women as Physicians,' by Dr. A. Jäderholm, of Stockholm, attracts attention in these days of "women's rights." The author gives an historical retrospect of the question, and describes its present status in America and in European countries. "In Finland," he says, "the question has lately been decided. An Imperial ukase of the 1st of May, 1871, declares that 'women who are desirous of devoting themselves to the calling of physicians are authorised to avail themselves of instruction at the medical faculty of the university,'—a regulation which seems to me to imply equality in studies and competency." "So far as I am aware," he adds, "the question has not been broached in the other *Scandinavian countries*." In Sweden, however, the country from which Dr. Jäderholm writes, it would appear that the matter has attracted a good deal of notice; and, though he is not aware that any Swedish ladies have as yet prepared to enter upon the troublesome and responsible calling of a physician, yet arrangements have been made in accordance with a letter from the king to the Royal College of Health, dated 3rd of June, 1870, to receive lady students in medicine (under certain restrictions) at the Carolinean Medico-Chirurgical Institute, which constitutes the medical faculty of the University of Upsala. According to these arrangements, lady students will have to pass the same examinations as ordinary students, but they are to receive separate instruction in anatomy.

Several other papers in this volume should be mentioned. Two from the pen of Gust. Retzius, Medical Licentiate, of Stockholm, deal with points in comparative and minute anatomy. Of these the first is on the internal limiting membrane of the retina, in connection with which the author describes peculiar supporting filaments, with trumpet-shaped terminal discs; in the second paper Retzius treats of the inner layer of the connective tissue of the eye. Professor Carl Santesson, in 'Some Remarks on the different Methods of Puncturing the Bladder,' strongly advocates an operation through

the rectum, performed with a fine trocar and canula. The instrument used by the author is a stilet of silver, with a steel point 20 centimètres in length; the canula, also of silver, being somewhat shorter, and having a diameter of about 1¼ millimètre. No untoward results, such as fistula or ulceration, seem to follow this operation. Professor Faye, of Christiania, reports and remarks on an interesting gynecological case. A large fibro-myoma, springing originally from the urinary bladder, filled up the true and false pelvis of a woman 41 years of age. Under these circumstances pregnancy occurred, and was followed by very difficult labour and death. Professor Carl Rossander, of Stockholm, writes on the treatment of amblyopia by subcutaneous strychnia-injections. At first he employed very weak solutions of nitrate of strychnia, but having gradually acquired greater confidence, he now uses a solution of 3 centigrammes of the nitrate in 5 grammes of water. The solution amounts to from 60 to 70 drops, and of it 7 to 8 drops may be injected, or 3 milligrammes ($\frac{1}{20}$ grain) of the alkaloid salt. Nine cases in which the remedy was tried are reported by Professor Rossander. The clinical histories after operation in five of these show that a rapidly occurring improvement *may* be transitory, and that hope should by no means too hastily be abandoned, even where no good result seems to follow the first injections.

To each number of the 'Nordiskt Medicinskt Arkiv' is appended a very full report of Scandinavian medical literature, dating from six months before. As an illustration, an abstract of a case detailed by Dr. Chr. Petersen, of 'scarlatina communicated in a letter,' may be cited. The original paper appeared in the 'Ugeskrift for Læger,' a medical journal published in Copenhagen. The reviewer, H. F. Trier, writes: "The author (Dr. Petersen) has made the observation, in the case of a girl, aged 17, who contracted scarlatina without the possibility of tracing the infection directly to any person, that a friend of the patient living several miles away had had the disease a month previously, and that this friend had regularly corresponded with the patient during the period of her desquamation. The author regards it as not impossible that scarlatina may be conveyed in this way, separate—perhaps microscopical—scales being thrust off the hands on to the paper during the writing of a letter, and the infection being thus carried to the address."

These quarterly reports embrace all subjects connected with (1) normal anatomy and physiology; (2), pathological anatomy, general pathology and therapeutics; (3), special pathology and therapeutics; (4), surgery; (5), psychiatry; (6), venereal affections and skin diseases; (7), obstetrics and gynecology; (8), pædiatrics; (9), pharmacology and toxicology; (10), questions of education, professional matters, state medicine, statistics, hygiene and epidemiology; and (11), reports from medical institutions.

2. The volume of the 'Hygiea' for 1871 is not unworthy to be compared with any of its predecessors in point of merit. In many of the monthly parts of the journal clinical histories are given under the heading 'Svensk Kasuistik,' or 'Cases in Swedish (Medical)

Practice.' Dr. W. Netzel contributes notes of three cases of ovariotomy to this department of the journal for March. In two instances the patients, aged 36 and 57 respectively, and both unmarried, recovered; but in the third, the patient, 32 years of age, and unmarried, died of peritonitis three days and a half after the operation. The titles of the other cases reported are annexed: "Medullary Sarcoma in the Left Thigh, with Secondary Depositions in the Lungs," by Professor C. Santesson; "Enterotomy in Ileus" (death), by Dr. Törnblom and Dr. Blix; "Invagination of the Intestine in an Infant," by Drs. A. Kjellberg and Blix; "Acute Inversion of the Uterus," by Dr. Sven Psilander; "Anchylosis of the left Elbow-joint, resection successfully performed," by Professor Santesson; "Spontaneous Rupture of the Uterus, followed by recovery," by Drs. Bolin and Netzel; "Vesico-vaginal and Vesico-uterine Fistula," by Dr. Sven Sköldberg; and "Contribution to Statistics of Ovariotomy," by Dr. August Falck.

The May number contains a paper by Dr. Oskar Sandahl on M'Boundou, or Icaja—a test or ordeal poison from Gabon, in West Africa. The author first met with the leaves and root of the plant at the Paris Exhibition of 1867, to which they had been sent by M. Griffon du Bellay, with this description: "Strychnos (species nova), racines et feuilles de M'Boundou du Cap Lopez, Casa ou Icaja des Gabonnais, employées comme poison d'épreuve dans les jugements à la côte du Gabon." The poison belongs to the class of spinal irritants; it acts with rapidity, but appears to be quickly eliminated from the system, so that its fatal effects may possibly be avoided by keeping up artificial respiration.

Dr. Wilh. Baggstedt read a paper on "Water Treatment in Iritis," before the Swedish Society of Physicians, on October 24th, 1871. As regards local measures, he has found moist *warm* applications to be very efficacious against pain and tenderness of the eye. A small oval linen compress, folded four to six times, is thoroughly moistened with water, and laid over the eye. A piece of gutta percha paper is next applied, and charpie, so as to fill up the orbit and to present a smooth surface, over which a bandage can be secured. Where this plan is properly carried out, (1) pain is assuaged, (2) the malady is shortened, and (3) the tendency to exacerbations during the attack is diminished.

As a contribution to literary medicine, Professor Curman's treatise on "Roman Baths and Finnish Bath-rooms" deserves a passing notice. The same may be said of Oscar Neyber's "Notes of a Journey to the Springs and Baths of Germany and Austria."

It may be mentioned that with January, 1872, a new series of the 'Hygiea' is commenced, under the editorship of Dr. A. Jäderholm, with the co-operation of Drs. A. Kjellberg, W. Netzel, C. Rossander, and E. Oedmansson. The letterpress is much improved, and a larger space is assigned for the translating, reviewing, and criticising of foreign medical literature.

3. At a meeting of the Swedish Society of Physicians, held on June 20th, 1871, Dr. Grähs introduced the question of the state of

vaccination in Stockholm. It would appear to have been of late years far from satisfactory, for while in the ten years ending 1867 43,793 children were born, in the same period only 20,576 children were vaccinated; in other words, only 46·9 per cent. of those born underwent the operation. Excluding the children who died as infants unvaccinated, and those who may have been vaccinated privately, we still have some 38 or 40 per cent. of all those born unprotected by vaccination. In the presence of this state of things the significant fact remains that smallpox, so far from decreasing, seems to become more and more prevalent. Into the smallpox hospital of Stockholm 2786 patients were admitted in the decenniad 1861—1870, of whom 282, or about 10 per cent., died. From the district physician's reports, and the records of the smallpox hospitals, it appears that in the years 1864 to 1870 inclusive more than 5000 cases altogether of the disease occurred, the deaths being 543. During the last of these years, 1870, the cases numbered 899, and the deaths 77, or a mortality of 8·5 per cent. From January to June inclusive of 1871, 546 cases, with 40 deaths, came under observation, either at home or in hospital. Hr. Grähs states that his object in bringing these facts under the notice of the Medical Society was, to urge the pressing necessity for an improved management of vaccination in the Swedish metropolis.

The 'Proceedings' of the Society, from which the foregoing information is culled, also contain a vast amount of valuable clinical material, together with weekly lists of the morbility of Stockholm. The latter are contributed by the physicians of the city from their public and private practice.

4. A prize treatise by Professor Aug. Almén, in answer to the question, "How should the goodness of a Drinking Water be decided from a sanitary point of view?" monopolises the third part of the 'New Transactions of the Swedish Medical Society' (second series). The Professor, who is one of the leading scientific medical men of Sweden, bases his answer on a comparative examination of no less than eighty natural waters throughout Scandinavia. In a supplement, the quality of the sea-water round the coasts of Sweden, Denmark, and Norway, and as far as Spitzbergen, is considered. It would be quite impossible to enter into details regarding this elaborate work. The results are given in tables and diagrams. Suffice it to say, that a useful text-book on water analysis might be compiled from those portions of the essay, which explain the methods of examination adopted and the preparation of the test solutions.

The fourth part of the 'Transactions' contains three important communications. Dr. Fr. Tholander criticises and illustrates some points in the doctrine of menstruation under the headings, "comparative nomenclature," "history," "anatomy," and "physiology." The second article is an address by Dr. Oskar Sandahl, delivered on the occasion of his laying down the presidentship of the Swedish Society of Physicians, October 4th, 1870. The subject is that of the development, prevention, and geographical distribution of intestinal parasites in man. In the same part, C. J. Holmström, army

surgeon, attached to the 1st battalion, reports to the Royal College of Health on the sanitary state of the military in the field during the late Franco-German war.

5. It may be well to remark, that the 'Upsala Proceedings' contain a series of papers, written in about equal proportion by both medical men and students. The latter are thus afforded an opportunity of placing on record the results of any original or test investigations they may happen to have made; and this, subject to the guidance and advice of men who hold the first rank as university professors or as practising physicians. An arrangement such as this must have very considerable advantages, but it is facilitated in Sweden, as it could not be in this country, by the prolonging of the period of undergraduate study through comparatively long series of years.

In Part IV of the 'Proceedings,' a paper on "Skin Grafting," from the pen of C. B. Mesterton, occurs. The author's remarks are illustrated by the clinical notes of six cases. The result in all of these go to support Hr. Mesterton's view—that, even where the transplanted piece of skin seems to disappear completely, the operation succeeds if only a few cells of the Malpighian layer of the cuticle remain possessed of life, so as to form a nucleus for a fresh epidermis formation. The operation of skin-grafting is graphically and physiologically described in the paper.

Frithiof Holmgren communicates an article on "Carnivorous Pigeons," and another on "Retinal Currents," to the fifth number of the 'Proceedings.' A group of essays on chemical, pharmaceutical, and toxicological investigations make up Part VI. Writing on the water-treatment of fevers, in the seventh number, Fr. Björnström gives a historical review of the subject. The chief indication for the use of the cold-water system is derived from thermometrical observations. Whenever the axilla temperature continues at 102° or upwards, Hr. Björnström believes that the patient should be cooled by means of water, applied repeatedly until the thermometer falls below the height mentioned. He concludes that cold-water treatment, while *proved* to be of special advantage in typhus, is probably "the best general method of treating febrile affections as a class." G. Kjellberg contributes an article on the indications for the administration of chloral hydrate in mental diseases. He gives the substance under the form of a plain solution: (1.) In *agitated melancholy*, where the patient is sleepless, or suffers from hallucinations; where a violent attack threatens, or a suicidal tendency is suspected. (2.) In *simple melancholy*, where less than four hours' sleep is enjoyed, or where hallucinations or suicidal tendencies exist; and (3.) in *acute mania*, where prodromata of a fresh attack appear. He combines the chloral with morphia: (1) in *acute mania*, towards the close of the attack, where the patient's state is variable —one hour calm and clear, the next confused—where violent delirium is followed by childish chattering, and exultation by quiet depression, critical sleep not setting in; and (2) in *chronic mania*, when an attack is protracted beyond the usual time, and a transi-

tion to permanent delirium is apprehended. The formula followed is that suggested by Dr. Hansen, of Schleswig: Of chloral hydrate, 5 to 10 grammes; acetate of morphia, ·03 to ·06 of a gramme; syrup, 30 grammes; peppermint water, 160 grammes. Of this mixture a table-spoonful is given every third hour.

Professor P. Hedenius, writing on "More Recent Investigations on Inflammation," has ably treated a confessedly intricate subject.

6. Among the original communications in the 'Norwegian Magazine of Medical Science for 1871,' we find one on the "Causes of Pneumonia," by Hr. Thorensen, of Eidsvold. The affection is of most frequent occurrence in the beginning of winter and in the month of April. Extreme variations in temperature and imprudence in dress are then the chief exciting causes of the malady. Contagion may also be regarded as an element of causation in some instances. Two or three small and circumscribed epidemics were observed by the author. In 1863 eleven persons lay ill of the disease within a short distance; in the winter of 1868 five persons who had been attending patients in pneumonia took the disease; and in the summer of 1869 the malady broke out in, and was confined to, a row of apartments near some glass-works. Hr. Thoresen does not look upon pneumonia as very contagious. He says, "We have not here to do with organisms which live, thrive, and maintain their contagious properties for a long time outside the human body; we have here only microscopical particles, which have escaped from the lining membrane and secretion of the attacked alveoli. These possess irritant properties, and introduced into a healthy lung are capable of giving rise to an inflammation."

In a communication on the "Dependence of Typhus Contagion on External Conditions"—an expression which corresponds to the older phrase, introduced by Sydenham, *epidemic constitution*—Dr. J. C. Holst believes that this inquiry is important with respect to the origin of isolated epidemics, and especially of sporadic cases of typhus, which often resist all the influences of isolation, evacuation, and disinfection. As factors in the inquiry, we find the season of the year, the varying contagious *force* of different epidemics, overcrowding, and bad hygienic conditions in general. The spread of typhus contagion through the air, even to considerable distances, the author looks upon as possible only with a greater degree of concentration in more severe epidemics, but he believes that the distance at which contagion may act still requires closer investigation.

Writing on the same subject, Dr. Aug. Koren expresses his conviction, from his own experience, that the throwing off of new-formed contagious products from the person of a typhus patient never begins before the third week of the disease. Typhus contagion flourishes most in an impure atmosphere, and the author has observed instances where a storm has suddenly checked or stopped an epidemic by "blowing away" the contagion. Of agents which tend to destroy the *materies morbi*, Dr. Koren places most reliance on sulphuric acid, while he is of opinion that carbolic acid and calcium carbolate are less efficient than chlorinated lime.

Professor P. Waage reports a chemico-legal investigation, which he made in a case of suspected phosphorus poisoning. A lady took three mouthfuls of tea, which tasted of lucifer matches; in a very short time violent pain in the stomach and lower part of the belly set in, accompanied by shiverings and nausea, but there was no vomiting. Diarrhœa followed, but next day all the symptoms had passed away. Professor Waage subjected a portion of the tea, prepared by pouring fresh water on the leaves remaining in the teapot from which the lady had drunk, to Mitscherlich's process for the detection of phosphorus. The fluid was found to contain about a sixth of a grain of phosphorus, together with a twentieth of a grain of arsenic. The source of both substances was lucifer matches.

The last two numbers of the magazine for 1871 contain portions of a very full report on "Practical Forensic Medicine," by Professor Voss, of Christiania. The data are derived from investigations carried out in accordance with a royal decree of the 9th of February, 1859, which required the Professor of Legal Medicine in the University to institute such medico-legal inquiries as were called for in the medical practice of Christiania.

Among the clinical records of the same volume are, "A Case of Ovariotomy, with successful result," by Dr. H. Vogt, of Bergen; "Peritonitis treated by Paracentesis," by Professor E. Winge; a report on "Electro-Therapeutics," for 1870, by A. Arndtsen; "Ileotyphus at Egersund and Omegn," by Albert Smith; and two cases of cystic ovarian tumour, with operation (in one instance successful), by J. Nicolaysen.

Six hospital reports are also contained in the volume under notice. They are:—1. "Report of the Medical Department of the Royal Hospital (Christiania) for 1869," by F. Stabell; 2. "Report of the Work done by the Lungegaard Hospital in the trienniad 1868—1870," by Head-Physician D. C. Danielssen; 3. "Summary Report of the Royal Hospital for 1870;" 4. "Report of the Surgical Department of the same for 1869," by Reserve-Physician Johan Hjort; 5. "Report of the Medical Department of the same for 1870" (up to October), by F. Stabell; and, 6, "Clinical Five-yearly Report of the Gaustad (Lunatic) Asylum," by Director Sandberg. The second of these includes a note on the use of carbolic acid in *spedalskhed* (leprosy). It was given internally, in a mixture at first of the strength of one grain of crystallized acid to an ounce of water—dose, a tablespoonful four times a day. Some thirty patients were treated thus for from three to eight months, but the results were dubious.

7. The 'Proceedings of the Norwegian Medical Society'—or, as it is also called, 'The Medical Society of Christiania'—contain reports of twenty ordinary and two extraordinary meetings, held during 1871. Dr. Bidenkap, medical officer of health (*Stadsphysikus*), on January 25th, gave an account of the "Epidemic Constitution of the Capital in the year 1870." Dr. A. Holst, on April 19, made a communication on the "Arrangements of the Army

Medical and Sanitary Department in Prussia." The cases from clinical practice brought forward by members of the society were—one of peritonitis with purulent exudation, in which paracentesis was three times performed, recovery finally taking place; one of cancer of the uterus and kidneys, in a child ten months old; one of congenital purpura; one of varicose aneurism of the heart; one of morbus Addisonii with morbid processes in both kidneys; and many others of an equally interesting character.

8. The second edition of the 'Norwegian Pharmacopœia' bears upon it the stamp of progress in a two-fold degree. The modern advances and requirements of science are recognised, and the cause of national Scandinavianism has been honorably borne in mind by the compilers of the work. Their object has been to assimilate it as far as possible to the kindred formularies of Sweden and Denmark, and in this direction a very large amount of success has been achieved. The metrical system of weights and measures is now for the first time adopted in lieu of the old Norwegian pound; and the pharmacopœias of Prussia, Germany, Austria, Great Britain and Ireland, France, and the United States of America, have been consulted as regards synonyms and the strength of many preparations. Following the modern fashion, the compilers have incorporated statements relating to the subject of materia medica with the alphabetical arrangement of the preparations and compounds, an uniformity of reference being thus secured. The work is in Latin, as the diversity between the Norwegian and Swedish tongues is too great to admit of either being used exclusively in a national pharmacopœia.

9. Hr. Synnestvedt's prize essay on "Bursæ Mucosæ" is an elaborate and original monograph on the subject. It is based on 106 dissections, carried out in the years 1867 and 1868. The author describes several mucous bursæ, which had not previously attracted the attention of anatomists; and his remarks are illustrated by four series of beautifully coloured plates, showing the anatomical relations of the principal bursæ mentioned in the work.

10. Army-Surgeon A. Holst's report bears witness to the anxiety always displayed by our Northern professional brethren to keep themselves *au courant* with the progress of medicine and sanitary science in other countries. Dr. Holst has made good use of the time spent by him at the seat of war in 1870—71, and presents his readers with a valuable description of the German army medical arrangements during the late campaign.

11. To the volume of the 'Hospitals-Tidende' for 1871 Dr. Edmund Hansen contributes a series of papers under the heading, "Ophthalmological Communications." The first in order of these articles is one on the "Treatment of Detachment of the Retina." The author, who has had considerable experience of this affection, having operated for it in some twenty instances, cannot place much reliance on the efficacy of operative measures. The methods he usually adopted were those recommended by Von Graefe and by Bowman. It is his conviction that we should not consider every improvement that may take place subsequent to the operation as

necessarily due to it. In no case was he fortunate enough to secure a permanent improvement, which might with certainty be attributed to the operation; sometimes an improvement would last for a couple of years, but without any essential change in the form or extent of the retinal separation. He would regard the efficacy of the operation as placed beyond doubt if we were successful in removing or diminishing the lesion *in a permanent manner*, and with accompanying improvement of function.

In another article Dr. Hansen gives his experience of atropia as a remedy in ophthalmic affections. Having alluded to the necessity of using a recent, perfectly neutralised solution of the alkaloid, he proceeds to speak of the untoward symptoms which may accompany its continued employment. Among these he enumerates an intolerance of the mucous membrane, shown by a certain amount of conjunctivitis in some cases, acute eczema of the eyelids with considerable œdema, diarrhœa, and ischuria. In conjunctival affections, atropia should certainly not be used. Even in vascular superficial keratitis, without accompanying ciliary injection, it is more likely to act as a hurtful irritant than to be of any advantage. In acute iritis, atropia is, beyond doubt, a sovereign remedy, unless in cases where the iris is very hyperæmic, with resulting chemosis. Under such circumstances, antiphlogistics should first be employed. In malignant forms of iritis, and in cases of old total posterior synechia, great caution should be observed, and the treatment by atropia suspended on the least sign of irritation. In glaucoma the remedy is altogether contra-indicated, as also in the deeper maladies—choroiditis, retinitis, and neuritis.

Dr. Hansen also reports three cases of *rupture of the choroid*. The first patient was a lad, aged 19, in whom the lesion was caused by the bursting of a gun. When examined a month after the accident, vision was found to be impeded by a diffused, tolerably dense, traumatic cataract. This was removed by peripheral linear extraction. After the operation, ophthalmoscopical investigation showed a rather long, very narrow streak in the choroid, situated between the optic disc, which was uninjured, and the macula lutea. The streak was partly of a white and partly of a rose-red colour. No trace of blood in the choroid or vitreous body existed. With the functional test there was no scotoma, or metamorphopsia. In the second case a man, aged 40, received a blow from a piece of firewood in the right eye. He lost the sight at once. In the choroid a long, narrow, whitish streak was found running obliquely upwards and outwards from the optic disc. Over this streak the retinal vessels ran uninjured. In the end, there was neither scotoma nor metamorphopsia. The third example of the lesion occurred in a smith, aged 34, who was kicked in the left eye by a horse. Three partly white, partly rose-coloured streaks, concentrically arranged round the optic disc, were noticed above, and to the outer side of it. At the upper limit of the second of these a dull blue, vesicular detachment of the retina, of considerable extent, was seen; its longest diameter forming a continuation of the second fissure in the

choroid, and the retinal vessels running across it in an arch. In a fortnight the detachment of the retina had disappeared; there was partial recovery of vision, with diminished acuteness. In subsequent remarks on these cases, Dr. Hansen describes the peculiar streaks or fissures in the choroid as a pathognomonic sign of rupture of that membrane. Among the accompanying lesions of the injury, he enumerates more or less extensive retinal detachments, even rupture of the retina (once observed), and frequently hæmorrhage in the choroid or vitreous body.

Dr. Vald. Rasmussen contributes two papers to the present volume of the 'Hospitals-Tidende.' The first of these is on "Hæmoptysis in Children." The author gives a historical review of the subject, and then reports at length two cases which had come under his own observation. These cases are typical of the two forms under which hæmoptysis generally shows itself in the course of pulmonary phthisis in children. The first is by the bursting of a small aneurism of a branch of the pulmonary artery, running in the wall of a cavity; and the second by ulceration of that artery from suppuration of a bronchial gland. An inquiry into the probable cause of the very rare occurrence of hæmoptysis in phthisical children of tender years concludes this original and instructive paper. Dr. Rasmussen reminds his readers that, in the "infiltration jaune" of Rilliet and Barthez, the form of phthisis most common in children, pulmonary cavities but rarely form; and that when such do form they are situated in the centre of cheesy, dry masses, which have long before compressed or obliterated the blood-vessels from which hæmoptysis might be looked for. Again, when cavities happen to form in children—which is very seldom—there is in them only a slight tendency to ulcerative processes, as Steffen has shown.

Dr. Rasmussen also writes on "Spina Bifida, and its Treatment." In treating the subject the author deals first with the anatomical conditions and the different forms of this affection. A considerable portion of the paper is devoted to the question of treatment. The first method alluded to is that originally proposed by Braynard, of capillary aspiration and subsequent injection with a solution of iodide of potassium and iodine. The cyst, having been emptied through an incision in the sound skin, at some little distance from the base of the tumour, is filled with a weak solution of iodide of potassium and iodine, at a temperature of from 86° to 95° Fahr. This is left in the sac for a few minutes, and then completely drawn off. A compress is afterwards applied. Dr. Rasmussen reports several cases, which he had successfully treated according to this plan, of which he expresses an unqualified approval. The other methods described are those by puncture, as recommended by Sir Astley Cooper and by Abernethy; by ligature, applicable where the tumour is pedunculated; by compression, only as a palliative measure; by excision, very dangerous, but occasionally successful; and by setons, the most dangerous proceeding of all. Our author sums up the indications for operative interference as follows:—(1) If the child is in other respects well made, and there is only

one tumour; (2) if the tumour is pedunculated; (3) if the superjacent skin is entire, and not ulcerated, and if it is homogeneous; (4) if compression exercised over all points of the tumour produces but little or no pain; (5) if the taxis of the tumour is unaccompanied by pain; (6) if fluctuation is uneven, being mediate to a greater degree at the apex of the tumour than elsewhere; and (7) if the tumour is simple, *i. e.*, if it does not include the spinal cord or nerves.

An article on "Endoscopy of Shot-wounds," from the pen of Dr. Chr. Fenger, of Copenhagen, appeared in the Journal of March 1st, 1871. During the summer of 1870 the author had been experimenting with the endoscope, and from observations of its use in the case of the urethra, was led to apply the principle of the instrument to the examination of shot wounds. The apparatus employed consisted of a brilliant petroleum lamp; a reflector, such as is used in laryngoscopy; specula of different sizes, for insertion into the wound, resembling the ordinary ear-specula, and a little less in diameter than the bullet sought for—each speculum being furnished with a tightly fitting button, which is drawn out when the instrument is inserted; a bent pincers, with doubly bent blades; bullet forceps of various kinds; and a bullet extractor and augur. Dr. Fenger made a series of experiments on horses and other animals. From his investigations he has found that foreign bodies carried into the shot wound could be readily distinguished from the vessels and removed through the speculum, and that bullets which have become wedged into the epiphyses of bones may be recognised, transfixed with the augur, and extracted. The speculum is well smeared with carbolic oil before insertion, and on drawing out the button the bottom of the wound is washed out with cold carbolised water. In the event of hæmorrhage occurring, a tent of charpie, dipped in solution of perchloride of iron, may be inserted into the speculum.

In a later number of the Journal, that for May 17th, Dr. Fenger reports two cases of operation with his instrument on wounded soldiers. He states, in conclusion, that examination with the endoscope did not in these instances present any peculiar practical difficulty, that it was perfectly possible to examine every portion of the walls of the wounds with accuracy, and that the operation was unattended by any special pain, hæmorrhage, or subsequent symptoms of irritation.

The volume contains many important clinical records in addition to the papers we have more especially noticed. Hr. C. Lange reports three cases of aphasia, with softening in the left cerebral hemisphere. Hr. Petersen, of Svendborg, brings forward an example of severe traumatic lesion of the vertebral column, the diagnosis being luxation upwards and forwards of both inferior oblique processes of the second dorsal vertebra. The patient, a labourer, fifty-three years of age, fell more than thirty feet on the back of his neck. On examination of his back a deformity was at once remarked, for the cervical and two upper dorsal vertebræ formed an angle with the remaining dorsal vertebræ, of which the third made the apex of the angle. The

patient recovered. Dr. Holmer communicates a case of eversion of the bladder, from congenital malformation. The lowest part of the abdominal wall bulged forward, and in it two small openings, the ureters, existed, from which the urine issued drop by drop.

This notice of the 'Hospitals-Tidende' would not be complete without an allusion to Dr. R. Bergh's elaborate "Report of the Second Division of the General Hospital (Copenhagen) for 1870."

12. 'Aphorisms on the Nature of Disease,' from the pen of a veteran writer, Professor O. Bang, and based on observations derived from a practice extending over fifty years, will amply repay perusal. Of the aphorisms, twenty-eight in number, we quote the first.

"Quæ ad organismum conservandum necessaria sunt,—*potentiæ salutares*—sive extrinsecus in cutem, sensus, vias respiratorias et digestivas agentia: ingesta, circumfusa, applicata; sive ipsius hominis voluntati sæpius adscribenda: gesta, excreta, ea *morborum* fiunt *causæ*, quotiescunque ratione ætatis, sexus, constitutionis corporis, tam hereditariæ, quam acquisitæ, a solitâ normâ discedunt; præter hæc alia, semper fere nociva dicenda—*potentiæ nocentes*—animi pathemata fortiora, vis externa, venena omnis generis, quorum non minimam partem efficiunt miasmata et contagia, nec non incongrua medela."

13. The part first of Dr. Stadfeldt's 'Lectures on Midwifery,' delivered at the Royal Lying-in-Hospital, Copenhagen, has just issued from the press in the form of a neat volume, convenient in size, and containing 144 pages. The present course of lectures is on "Disproportions in Roominess of the parts engaged in Childbirth." The author treats his subject very fully, and adopts an admirable arrangement. He proposes to deal firstly with diminution of pelvic capacity, whether arising from (A) narrowness of the pelvis itself, or from (B) tumours filling up its cavity; secondly, with narrowing in the soft parts of the genital tract, that is, in the os uteri, vagina, and vulva; thirdly, with disproportions resulting from the unusual size or development of the fœtus; and, fourthly and lastly, with disproportions due to faulty positions of the fœtus.

In the present volume only the first division of his subject—diminished pelvic capacity—is handled by Dr. Stadfeldt. In speaking of malformations of the pelvis, he gives a very interesting *résume* of the cases lately met with in the institution to which he is attached. A brief abstract of this *résumé* will, perhaps, be acceptable.

In the trienniad 1869-70-71, 3283 patients were delivered in the Royal Lying-in-Hospital. Pelvic narrowing was noted in 104 instances, that is, 3·2 per cent. The smallest diameter has varied between 2¼ and 3¼ inches. In 8 cases the form of pelvis belonged to the rarer types (coxalgic-asymmetrical, 4 examples; cyphotic, 2 examples; oblique-anchylosed of Naëgele, 1 example; and funnel-shaped, 1 example). Eight deaths occurred among these 104 patients. Of those who died, 2 were delivered with the forceps, 2 by cephalotomy, and 1 by the Cæsarian section. Twenty-one deaths took place among the children, or a mortality of about 20 per cent. Of the fatal cases, 1 child was born by the efforts of nature, 7 chil-

dren with the aid of the forceps; on 6 cephalotomy was practised, 2 were delivered by traction, and 5 with turning by the feet and traction. The labour was ended through operative interference in 49 of the 104 cases. The forceps was used 23 times; traction on the feet 5 times, and, after turning, 14 times; cephalotomy, as before stated, was resorted to on 6 occasions, and Cæsarian section was once performed. The *physiological asymmetry* of the human head, first described by the author ('Dub. Quarterly Journal,' August, 1864), played a prominent *rôle* in the case of the children thus born from deformed pelves. Among tumours in the pelvic cavity Dr. Stadfeldt describes ovarian enlargements, subperitoneal fibroid growths or fibro-myomata, vaginal cystocele, vesical calculus, neoplasmata in the vesical walls, intestinal or omental herniæ, rectocele, cancer of the rectum, large cysts engaging the pelvic connective tissue, and hæmatoma of the vaginal walls. The volume concludes with an account of the operations of cephalotomy and Cæsarian section, the first being considered under the heads Perforation and Cephalotripsy. The indications for the three operations respectively are given in a clear and pithy manner. Obstetricians will, doubtless, look forward with pleasure to the appearance of the remaining two parts of Dr. Stadfeldt's work.

14. The number of the 'Ugeskrift for Læger,' for May 18th, 1872, which has lately come to hand, contains some remarks on "Sterility," by Professor Stadfeldt. The author of the paper confines his attention to the subject of vaginismus as a cause of sterility, and deals with the question of the origin of this lesion and its treatment. Some interesting cases are detailed in illustration. A rather novel cause of vaginismus is described. The contracted remains of the hymen are shown to produce reflex irritation of the sphincter vaginæ. Under such circumstances the removal of the exciting cause is an essential part of the treatment.

In the same periodical Dr. F. Trier writes on the present epidemic of smallpox in Copenhagen. Of this valuable article some account is given further on.

Vital Statistics.

Hr. Wistrand's admirable Reports, in the numbers of the 'Hygiea' for June, July, and December, 1871, afford important information as to the health of Sweden during the years 1869 and 1870.

1869.—In an epidemiological respect this year shows that *measles* assumed proportions of an epidemic more general in extent and frequency than any since 1862. *Typhus* and *gastric fevers* continued to be unusually prevalent, although rather less so than in the preceding year. *Scarlatina*, and, in some degree, *diphtheria* also, again increased, while *ague* was more common than it had been for six years before. *Inflammatory* and *catarrhal fevers* were more generally and frequently met with. On the other hand, *hooping-cough*, *smallpox*, *diarrhœa*, and *endemic cholera*, were less prevalent. *Dysentery*, *croup*, *cerebro-spinal meningitis*, and *mumps*, were but rarely observed. Of *smallpox*, 6688 cases, with 750 deaths, occurred throughout the kingdom, against 6951 cases, with 740 deaths, in 1868.

Measles gave 39,679 cases, with 2380 deaths, against 10,889 cases, with 520 deaths, in 1868.

1870.—The epidemic of ***measles*** just alluded to ceased early in the year, the cases in the whole twelve months numbering only 2490, with 160 deaths. Some diminution in the number of cases of ***typhus*** and ***gastric*** fevers was noticed. A marked increase in ***scarlatina*** occurred (12,688 cases, with 2459 deaths, against 9245 cases, with 2266 deaths, in 1869); though, as may be seen, the mortality from the disease was not relatively so great. ***Mumps***, ***dysentery***, ***diarrhœa***, and ***cholerine***, also ***pneumonia*** and ***catarrhal fevers*** were somewhat more prevalent. ***Ague*** was as in the preceding year. ***Hooping-cough***, however, and ***smallpox*** showed a marked decrease, while ***cerebro-spinal meningitis***, ***croup***, and ***diphtheria***, were comparatively rarely met with. The cases of ***smallpox*** were 3722 in number, with 397 deaths, being but little more than one half the mortality from this cause of the previous year.

Equally interesting facts concerning the health of Norway of late years are derived from abstracts (in the 'Northern Medical Archives') of articles from the 'Norsk Magazin,' and of the Reports published annually by the Ministry of the Interior.

The 'Official Report of the Health of Norway in 1868' shows an annual mortality in a population of 1,725,081 of 18·40 (or, including the stillborn, of 19·52) per 1000 living. The corresponding figures for 1867 were 18·47 and 19·62 respectively. The number of deaths was 31,737, of stillbirths 1934. The number of births exceeded that of deaths by 19,924. Among 11,619 deaths, the causes of which were assigned, pneumonia induced 958, cholerine and diarrhœa 571, measles 373, scarlatina 435, bronchitis (acute and chronic) 514, apoplexy 291, and leprosy 211. Consumption (*tæring*) heads the list with 1627 deaths, and typhus fever stands third with 770 deaths. The last-named disease was almost epidemic, and measles was largely prevalent in many districts of the kingdom.

Information, though not of an official kind, respecting 1869 and 1870, is conveyed in 'Reports to the Norwegian Medical Society,' by Drs. Bidenkap and Kjerulf. The mortality of the whole kingdom was unusually low in both years. While the yearly average was 18 per 1000 living, in 1869 the rate fell to 17 per 1000, and in 1870 still further to 16·3 per 1000. This may be attributed to—(1) a lower birth-rate (the greatest mortality being among the lately born); (2) the good harvest of 1868; and (3) a favorable epidemic constitution. It should be mentioned that 46,818 individuals were vaccinated in 1868.

Dr. F. Trier, in the 'Ugeskrift for Læger' for May 18th, 1872, gives an account of the existing epidemic of smallpox in Copenhagen. He states that the present is the eighth occasion, since the introduction of compulsory vaccination in 1810, on which the disease has assumed epidemical proportions.

The still prevailing outbreak may be said to have commenced on the 1st of November last, the number of cases reported weekly rising quickly about that date. In the nine weeks ending January 2nd,

1872, 261 cases, with 18 deaths, were recorded. The number of new cases per week were subsequently 66, 86, 66, and 79, in January; 73, 74, 87, and 91, in February (the last number being the highest recorded in one week during the epidemic); 75, 81, 64, and 80, in March; 60, 49, 62, and 47, in April; and 27 and 37 in the two first weeks of May. In January, the deaths were 18; in February, 23; in March and April together, 45; in the four months, 86 in all.

In the first half year of the epidemic (1st November, 1871, to 30th April, 1872,) the cases have numbered 1481, with 104 deaths, or a mortality of 7·02 per cent. The population of the Danish capital being 181,291 (1870), Dr. Trier concludes that the present is more fatal than any of the seven preceding epidemics, and that only one of the latter was more prevalent. It should be mentioned that his conclusions are based on the supposition that the epidemic has yet many months to run.

Before concluding this review, I wish to remark that, so far as I have been able to learn, no *epidemic* of smallpox has as yet manifested itself in Sweden, or, indeed, in Norway. From the lists of morbility published weekly in Stockholm, Upsala, and Christiania, the numbers of cases of variola and varioloid appear not to have materially increased during the past winter. In the city of Stockholm, in the eight weeks ending February 10th, 1872, 146 cases of variola came under notice in public and private practice. The cases in the corresponding period last year numbered 177, and in that of the year before 224. These figures show a very perceptible *decrease* in the prevalence of the disease. It remains to be seen whether an epidemic is yet to visit the countries in question, or whether the danger is already past.

Appendix of Journals, Papers, and Works analysed in the above Report.

1. 'Nordiskt Medicinskt Arkiv.' Redigeradt af Dr. Axel Key. Tredje bandet. Med 7 Taflor och flere Träsnitt. 1871. Stockholm: Samson och Wallin.
'Northern Archives of Medicine.' Edited by Dr. Axel Key. Third volume. With 7 plates and several woodcuts. 8vo.

2. 'Hygiea: Medicinsk och Farmaceutisk Månads-skrift.' Trettiondetredje bandet. No. 1—12. Januari—December, 1871.
'Hygiea: A Monthly Journal of Medicine and Pharmacy.' Thirty-third volume. Nos. 1—12. January to December (inclusive), 1871.

3. Förhandlingar vid Svenska Läkare-sällskapets Sammankomster, år 1871." Protokollsförande, Dr. Edholm.
'Proceedings of the Meetings of the Swedish Society of Physicians in 1871.' Edited by Dr. Edholm, Secretary.

4. 'Svenska Läkare-sällskapet Nya Handlingar. Serien II. Delarne III. och IV.
'New Transactions of the Swedish Society of Physicians.' Second series. Parts III and IV.

5. 'Upsala Läkareförenings Förhandlingar.' Sjette bandet. Häftet, 1—8. Upsala, 1870 och 1871.
'Proceedings of the Upsala Medical Association.' Vol. VI. Parts 1—8 inclusive. Upsala, 1870 and 1871.

6. 'Norsk Magazin for Lægevidenskaben.' Udgivet af det medicinske Sels-

kab i Christiania. Tredie Række, første Bind. Christiania: Th. Steens. 1871. 8vo, pp. 807.

'Norwegian Magazine of Medical Science.' Published by the Medical Society of Christiania. Third series. Volume I.

7. Forhandlinger i det Norske medicinske Selskab i 1871.' Christiania: Steens. 1871.

'Proceedings of the Norwegian Medical Society in 1871.' 8vo, pp. 245.

8. 'Pharmacopœa Norvegica.' Editio Altera. Regia auctoritate edita. Christianiæ: 1870. Impensis Alb. Cammermeyer. Typis: H. J. Jensen.

9. 'En anatomisk Beskrivelse af de paa Over-og Under Extremiteterne forekommende Bursæ Mucosæ, &c.' Pris-belœnnet Afhandling af A. S. D. Synnestvedt, Stud. Med. Christiana: 1869.

'An Anatomical Description of the Bursæ Mucosæ occurring in the Upper and Lower Extremities.' A prize treatise by A. S. D. Synnestvedt, Student of Medicine. Edited by Dr. J. Voss, Professor of Anatomy. Christiania: 1869.

10. 'Uddrag af Indberetning til Armeekommandoen om en Vinteren, 1870—1871, med offentligt Stipendium foretagen Reise til Tyskland og Frankrig.' Af Korpslæge A. Holst. Christiania.

'Extract from a Report to the War Department respecting a Journey to Germany and France in the Winter of 1870—1871, undertaken at the Public Expense.' By Army-Surgeon A. Holst.

11. 'Hospitals Tidende: Optegnelser af Praktisk Lægekunst, fra Ind-og Udlandet.' 1871. 14de Aargang. Kjöbenhavn: Jacob Lund.

'Hospital Gazette: Notes on Practical Medical Science—Home and Foreign.' 14th annual volume. Copenhagen: Jacob Lund. Folio, pp. 208.

12. 'Aphorismi de Nosogenesi.' Autore O. Bang. Havniæ, 1870.

13. 'Det mekaniske Misforhold under Fœdslen og dets Behandling.' Af Dr. A. Stadfeldt. Foerste hefte. Kjöbenhavn: C. A. Reitzels Forlag, 1872.

'Mechanical Disproportion during Parturition, and its Treatment.' By Dr. A. Stadfeldt. Part I. Copenhagen: Published by C. A. Reitzel. 1872.

14. 'Ugeskrift for Læger.' 3dje Række, XIII. Nr. 23, 24. D. 18 Maj, 1872. Kjöbenhavn.

'Weekly Medical Journal.' 3rd series. Vol. XIII. Nos. 23 and 24. May 18th, 1872. Copenhagen.

REPORT ON OBSTETRICS AND THE DISEASES OF WOMEN AND CHILDREN.

By W. S. Playfair, M.D., F.R.C.P.,

Professor of Obstetric Medicine in King's College, and Physician for the Diseases of Women and Children to King's College Hospital.

1.—The Non-Pregnant State.

1. *A Contribution to the Study of Membranous Dysmenorrhœa.* By MM. Henri Michaud and F. Labadie Lagrave.
2. *Intermenstrual, or Intermediate Dysmenorrhœa.* By Dr. W. O. Priestley.
3. *Vicarious Menstruation by means of Epistaxis.* By Dr. Otto Obermeier.
4. *On some Forms of Menorrhagia.* By Dr. Lombe Athill.
5. *On Pelvic Hæmatocele.* By Dr. Meadows.
6. *On Prolapse of the Female Genital Organs.* By Dr. Konrad.

7. *On Procidentia of the Pelvic Viscera.* By Dr. J. MATTHEWS DUNCAN.
8. *Contributions to the Diagnosis by Puncture, of Collections of Fluid in the Abdominal Cavity.* By Professor SPIEGELBERG.
9. *Case of Wound of Intestine during Ovariotomy, with Recovery.* By Mr. CHRISTOPHER HEATH.
10. *On Gastrotomy in Cases of Fibrous and Fibro-Cystic Tumours of the Uterus, and on a Case of Spontaneous Absorption of a Uterine Fibroid.* By M. PÉAN.
11. *On Spontaneous Absorption of Fibroid Tumours of the Uterus.* By M. GUENIOT.
12. *On the Curves of the Developed Genital Passages.* By Dr. MATTHEWS DUNCAN.
13. *On the Diagnosis of the First Stage of Cancer of the Uterus.* By Professor SPIEGELBERG.
14. *On Uterine Paraplegia.* By M. PETER.
15. *On Vaginismus.* By Professor F. PARVIN.
16. *On the Influence of Marriage on Health.* By M. BERTILLON.
17. *On Intra-Uterine Injections.* By Dr. A. GUICHARD.

1. The authors continue their elaborate paper on this subject. They distinguish two varieties of membranous dysmenorrhœa:

1st. Membranous, or exfoliative dysmenorrhœa.
2nd. Pseudo-membranous, or exudative.

The former depends on a true exfoliation, total or partial, of the lining membrane of the uterus, the latter is of an inflammatory nature, and allied to a croupous endo-metritis.

They dispute the ordinary theory, that this disease depends only on an intense congestion of the uterine mucous membrane, which causes it to be detached in masses, or in pieces of considerable size, instead of in minute shreds. They believe that analogy with other mucous membranes, such as those lining the respiratory and urinary tracts, would lead us to admit the existence of a true exudative form of inflammation resulting in the formation of false membranes.

This would lead us back to a theory held by many authors as to the inflammatory origin of these membranes, and which only comparatively recently gave place to the more modern views on the subject. While admitting, however, that the expelled membrane often consists chiefly of the exfoliated lining of the uterus, the authors believe that this, as well as the other variety of membranous dysmenorrhœa, depends on the presence of endo-metritis. According to the site, nature, and intensity of the inflammation, the expelled membranes present the different characters previously described; they consider membranous dysmenorrhœa, therefore, to be not a special disease, but a symptomatic expression of various inflammatory affections of the lining membrane of the uterus, deriving its special character from the periodicity of the menstrual functions, and the exaggeration of physiological phenomena determined by it. —*Arch. Gén. de Médecine*, December, 1871.

2. In a paper read before the Royal Medical and Chirurgical

Society, Dr. Priestley referred to a peculiar form of pain connected with the menstrual function, which comes on with regularity about the mid-period between the two menstruations, and is probably dependent on organic changes connected with the production of the catamenia. The pain commonly comes on about fourteen days after a menstrual period, and, after lasting a variable number of days, cease before the next expected period. The precise explanation of this is not apparent, but the author suggests that thickening of the indusium might lead to undue vascular excitement, and impede the advance of the ova to the surface in their attempts to attain maturity.

3. Dr. Otto Obermeier relates an instance of this occurring in a servant, aged 24. The first catamenia appeared at 15, and were very profuse. Epistaxis occurred the following month, and appeared regularly for several years, until she became pregnant in March, 1870, when it ceased, but again returned six weeks after parturition, and continued regularly for some months, although not so freely, when conception again took place. The epistaxis usually lasted three days, and was accompanied by malaise, feeling of giddiness, muscæ volitantes, &c.—*Virchow's Arch. für Path. Anat. und Phys.*, &c., Band 54, heft 3, 1872.

4. Dr. Atthill discusses the treatment most applicable to those forms of menorrhagia which occur in connection with, or are dependent on, sub-involution of the uterus, or granular ulceration of the cervix, or on an unhealthy condition of the mucous membrane lining the body of the uterus. Dr. Atthill considers defective involution of the uterus to be a prominent cause of excessive menstruation. Connected with this a granular condition of the mucous membrane of the uterus is often found, which increases the previously existing tendency to hæmorrhage. In the former case he thinks the best treatment is to lodge a piece of solid nitrate of silver in the cavity of the uterus, by means of a uterine *porte caustique*. This seems to act by stimulating the uterus to set up that healthy action by which it regains its normal size after pregnancy has terminated. In that form depending on a granular and hypertrophied condition of the mucous membrane, Dr. Atthill advises the cauterisation of the diseased surface with fuming nitric acid. He has adopted this plan in numerous cases with the most satisfactory results. Before this is done the os must be dilated by laminaria tents.—*Dublin Quarterly Journal*, November, 1871.

5. Dr. Meadows contributes an elaborate paper on this subject, in which he argues that the expectant treatment of hematocele is not the best, and that far more favorable results follow the practice of puncturing, so as to get rid of a portion of the effused blood. He says, "It is impossible, I think, after studying these facts, to resist the conclusion which they seem to demonstrate most forcibly, namely, that the practice which has hitherto been recommended, of leaving these cases to nature, in the hope that the blood would be absorbed, and that all the physician can do is to favour that process by rest,

warm applications, stimulating embrocations, opium, mercury, local depletion, &c., is altogether unsound and contrary to experience, and that we ought rather to regard the blood thus effused as a foreign body, which is to be got rid of by the quickest way possible, viz., by tapping." The author advises that this tapping should not be performed at once, but that a certain time—a month at least—should be allowed to pass after the effusion of the blood, in order that some kind of encysting process should take place.—*Obstetrical Transactions*, vol. xiii.

6. This paper embodies the views of Professor Spiegelberg, as well as those of the author, who is his pupil. He maintains that prolapse of the vagina is always the most important feature in any prolapse of the female genital organs, to which prolapse of the uterus is always a sequel. Enlargement of the vaginal portion of the uterus (circular hypertrophy of Spiegelberg) is of no diagnostic value, and is only a sequence of mechanical irritation. He believes its importance to have been over-estimated. He divides his treatment into—*a*, palliative; *b*, radical. The former consists in the retention of the uterus in its proper position either by Myer's ring pessary or Hodge's modification of it. For the radical cure, a combination of Hugier's and Dieffenbach's operations is recommended.

An animated discussion followed the reading of this paper, in which the author's views on the causation of the affection were controverted, Dr. Phillips, Barnes, and others maintaining that morbid conditions of the uterus itself, producing over-weighting of that organ, were important factors in the production of prolapse.—*Obstet. Trans.*, vol. xiii.

7. Dr. Duncan enters very fully into this subject, and, under the title of procidentia, he refers to procidence of the uterus, vaginal cystocele, vaginal rectocele, or a combination of them. The bladder, he says, is always procident, if the uterus is so, on account of the intimate connection between them; the connection of the rectum and vagina being much less intimate than that of the bladder and vagina, the bowel is sometimes, even in cases of great uterine procidentia, almost unaffected. The vagina is the organ most affected in procidentia, as it covers or invests the whole of the displaced viscera.

The author is convinced that the inflammations and ulcerations commonly found on the cervix, in cases of complete prolapse, are not the result of friction and urinous irrigation, as is generally stated. Many of the so-called ulcerations are simply patches of inflammation covered with a pellicle of lymph, like diphtheritic membrane.

Dr. Duncan appears not to attach the same importance to hypertrophic elongation of the cervix that some pathologists do, although he recognises its frequent occurrence. He describes it as being to some extent due to stretching of the cervical tissue, and he has seen a uterus which was elongated to six inches reduced to only four inches, after a week of replacement and rest in bed.

With regard to the etiology of procidentia, Dr. Duncan believes the causes leading to its production to be simply mechanical, gene-

rally of long standing, and to have close relations to those producing herniæ:—"The forces which in their just equipoise maintain the uterus in its natural position, and maintain likewise the other pelvic viscera, may be deranged in two ways. The elevating or retaining forces may be in excess; and then the uterus and other organs are drawn high up, unnaturally elevated. This is often seen, so far as the uterus is concerned, in old age. The retaining forces may be diminished, or the expelling forces may be predominant; then the uterus and other organs will descend."

Laceration of the perinæum is not a cause of prolapse, but only facilitates its occurrence by shortening the route which the descending organs have to take. With regard to treatment, Dr. Duncan recommends, in severe cases, an ordinary T bandage, a modification of the stem pessary, and, if necessary, the restoration of the perinæum. —*Edin. Med. Journal*, January, 1872.

8. Professor Otto Spiegelberg narrates the details of three cases where the diagnosis by the ordinary means at disposal left the question still in doubt, but examination of the fluid abstracted enabled him to clear up the diagnosis.

Case 1.—Cyst occupying the centre of lower and middle abdomen, of doubtful origin. Evacuation of pus through puncture. Tentative incision and rupture of the cyst after tearing away of adjacent adhesions. Extrusion of an echinococcus membrane; retroperitoneal echinococcus sac on left side. Partial excision. Cure through disintegration of the sac after long-standing suppuration.

Case 2.—Large retroperitoneal and mesenteric sarcoma reaching down into pelvis, simulating an ovarian cyst by means of numerous saccular ascitic spaces, and secondary pedunculated adhesions, with the fundus uteri. Disclosure through puncture. Death from marasmus and œdema of the lungs.

Case 3.—Cystoid tumour of doubtful origin; cylindrical epithelium in the fluid drawn off; ovarian in character. Ovariotomy, incomplete, on account of extensive internal adhesion to the parietes. Death through decomposition of the cyst.

He holds the opinion that the diagnostic puncture does not of course always give a positive explanation of the origin of the fluid, but that where it does this the explanation is decisive against the doubtful result of the physical, &c., examination, and believes in its accurate value, and, according to our present knowledge, it is indisputably a means of rendering our diagnosis more precise.—*Arch. f. Gyn.*, iii, 2, 1872.

9. Mr. Heath relates an interesting case of ovariotomy in which an empty coil of intestine, closely adherent to the abdominal wall, was accidentally divided, while the first incision was being enlarged with scissors. The bowel was divided through three quarters of its circumference. The cyst was removed with considerable difficulty, on account of adhesions. The divided gut was attached to the skin with silk sutures, forming an artificial anus. The patient made a perfectly good recovery, fæces and flatus passing

by the artificial opening, and solid motions per anum. The patient was convalescent in a month. The use of a belt and air pad satisfactorily retained all fæcal matters, and the patient had now regular stools per anum.—*Clinical Society*, Nov. 10th, 1871.

10. The author contributes some cases of great interest in which gastrotomy was performed. He considers the operation to be only justifiable—

a. When serious symptoms are produced on account of the great size of the tumour and its pressure on the surrounding parts.

b. When there is profuse metrorrhagia imperilling the life of the patient.

c. When the presence of the tumour causes intolerable pain either in the abdomen or inferior extremities, thereby entirely destroying the patient's comfort in life.

In suitable cases he believes the difficulties and dangers not to be greater than in ovariotomy. In two of the three cases in which M. Péan operated successfully there were large intra-uterine fibroids, the removal of which necessitated the removal also of the greater portion of the uterus, both ovaries, and the fallopian tubes. The third tumour was found to have only slight uterine attachments, and the organ was therefore left untouched. In the two cases of uterine fibroid the abdominal incision reached from the pubes to above the umbilicus, and was found to be sufficiently large to allow the bulk of the tumour to be extracted through it. The bulk of the tumours was excised after their base, which formed a narrowish neck, had been transfixed by a double metallic ligature, which was firmly held on either side. The third was an enormous fibroid, apparently originating in the right broad ligament, but filling the whole abdominal cavity, and being extensively adherent to the surrounding structures in every direction, but so complicated were these adhesions that the operation lasted upwards of three hours. The patient, however, made a perfect recovery.

M. Péan has also operated on two cases of fibro-cystic uterine tumour. One of these has been previously published; the second was operated on during the siege of Paris, and under the most unfavorable conditions. The whole uterus was removed. The patient, however, made an excellent recovery.

M. Péan incidentally relates the particulars of a case of spontaneous absorption of a uterine fibroid. The tumour was of enormous size, and filled nearly the *whole abdominal cavity*. Five years ago M. Péan saw the lady in company with other medical men, and the nature of the tumour was carefully ascertained. Last June he again saw her, and found that the tumour had entirely disappeared.—*Gaz. des Hôp.*, Nov. and Dec., 1871.

11. The author maintains that the frequent examples now recorded of this occurrence enable us to consider it as definitely proved. He believes that the puerperal state only exceptionally exercises an influence in producing it. The tumours seem to disappear by a process of fatty degeneration of their structure.

Remedies known to favour fatty changes, such as arsenic, phosphoric acid, &c., are therefore indicated.—*Union Méd.*, March, 1872.

12. Dr. Duncan discusses this subject in a paper of considerable length. The first curve to which he directs attention is that said to exist at the brim of the pelvis, and to have its convexity directed downwards and forwards. This he does not admit to exist. The views of Schulze on this point are discussed, and the reasons for differing from them given. Among the difficulties in the way of admitting the presence of this curve of the natural promontory is that if this exists, the axis of the genital passages, regarded in the antero-posterior vertical plane, has the shape of a roman S; its first or upper curve, the curve of the natural promontory, having its concavity looking backwards; its second, and universally recognised curve, having its concavity looking forwards. The author believes we are nearer the truth in thinking that in the antero-posterior vertical plane, the genital passage has only one curve, having the concavity of its axis looking forwards.

II. The second curve is also situated at the brim, and is indicated by the deflection of the uterus from the medial line to the right or left. It is probable that the angle of deviation of the axis of the uterus from the axis of the brim of the pelvis has important practical and physiological bearings as yet but little worked out. The author proceeds to show the probable influence of this curve in the production of face presentation.

III. The last curve is the great one in the antero-posterior vertical plane, which begins about the middle of the third bone of the sacrum, and extends through the outlet of the ligamentous pelvis to the outlet of the soft parts. It forms a curve whose amount of bending varies from 60° to 150°. In connection with this curve the author studies the development of the lower parts of the genital passages, the greater development posteriorly where the force is more particularly or strongly applied than anteriorly, where there is little more than counter pressure, or pressure against a fixed wall, and chiefly during the temporary abeyance of the power of parturition. There is to be noted, also, in connection with this curve, the inevitable tendency of the force of labour, not merely to distend the perineum, but also to rupture it centrally, which leaves, in the author's mind, no possible doubt as to the expediency of supporting it.—*Med. Times and Gaz.*, March 2nd, 1872.

13. Professor Otto Spiegelberg, of Breslau, in an able paper, discusses the difficulties of recognising the first local commencement of uterine cancer. From extended opportunities of observation and from anatomical considerations, he believes it possible to determine the question from the peculiar hardness of the cervix, the condition of the covering of the mucous membrane, and finally, by the reaction to dilatation by sponge tents. He has never had occasion to regret relying upon these. The hardness and unyielding nature of the cancerous deposition compared with diffuse or circumscribed hyperplasia, the so-called simple or benign induration, is well known and

very characteristic; and the rarity of the former compared with the latter condition will also assist us; still the limits are difficult to assign, and he therefore relies more upon the two other conditions. In cancerous degeneration the mucous membrane covering the cervix is always immobile, and bound down firmly to the underlying tissue, whilst in hyperplastic thickening and induration this is not the case. By passing a sponge-tent, and noticing the change induced by its swelling, we find in simple hyperplasia that the tissue dilates regularly, becomes looser, softer, and thinner, whilst in cancer it remains unchanged, rigid, inflexible, and is not stretched.

Waldeyer shows that cancer of the cervix uteri is developed in the interpapillary depressions of the rete Malpighii of the mucous membrane, or in very rare cases from the glands of the cervical canal (alveolar carcinoma).

In the majority of the cases, amputation is inadmissible when they present themselves for observation, and then any operative interference in the way of cutting, &c., produces severe hæmorrhage, and the disease progresses more rapidly after. His advice is to operate very early, or where this is no longer possible, to treat symptoms.—*Archiv f. Gyn.*, iii, 2, 1872.

14. The author maintains that the cases of paraplegia, often met with in connection with uterine disease, cannot be explained on the assumption of their being due to reflex action. This explanation he believes to be much too indefinite to be of any use, and it does not explain why the disease is met with in certain cases of uterine disease only, while in certain other precisely similar cases nothing of the sort is seen. Nor does he believe that pressure on the nerve branches in the pelvic cavity, by tumours or inflammatory products, has anything to do with its production, since pressure on nerves always produces pain—a symptom generally absent; and pressure from tumours, &c., exists without producing any such effect. He believes that the constitutional tendency of the patient is always at the bottom of these cases. It only shows itself when some uterine derangement occurs in patients predisposed to nervous affections by their own previous state of health or hereditary tendency.—*Gaz. des Hôp.*, January, 1872.

15. The author states that, undoubtedly, the majority of cases of vaginismus can be cured without resorting to the knife. The removal of the cause is frequently sufficient in recent cases. The application of local sedatives may answer in others; still, others may be cured by dilatation, either *gradual*, as with glass bougies or gum elastic bags distended with air or water, or *abrupt*, as performed in the manner originally proposed by Récamier for spasmodic contraction of the sphincter ani.—*American Practitioner*, August, 1871.

16. The author has read an elaborate statistical paper on the comparative effects of the celibate and married states on the health. The general result is to show that the influence of marriage on the health is greater than is generally supposed. Thus, in males, the average per-centage of deaths per 1000 is 6 for the married, 10 for

celibates, and 22 for widowers. The latter figures point, as the author thinks, to the evil effects of the single life on those who have been long married. At all ages, married men furnish a less proportion of deaths per 1000 than either celibates or widowers. There is, however, an exception to this rule under twenty years of age; the deaths of celibates at this age average 7 per 1000, of married 50. This seems to point strongly to the evil effects of marriage in those whose growth and development are not completed. The advantage of the married life is not so marked in the female; it is hardly perceptible until after 30 years of age; after 30 the deaths in the unmarried are 11 per 1000, and in the married 9. This difference increases steadily up to 55 years. Then at 50 the deaths of married were only 15 to 16 per 1000, of the unmarried and widows from 26 to 27. Under 20 the evil effects of marriage are not so remarkable as in males, being 7·53 per 1000 for the unmarried, as against 11·86 for the married. The difference of results between male and female the author is inclined to attribute to the somewhat increased mortality in younger women from childbirth.

The facts may be summed up by saying that the expectation of life in a married man between the age of 20 and 25 is 40 years, of an unmarried 35; in a woman marrying between the same ages also 40 years, as against 36 for a spinster.

The author then proceeds to consider the effects of marriage on crime, which are remarkable. Taking 100 to represent the criminality of celibates, that of the married is 49 for crimes against the person, 45 for crimes against property; and not only is there this enormous difference, but in the gradual diminution of crime in France since 1840 the married have had the greatest share. The favorable influence of marriage, in regard to suicide and insanity, is also shown.—*Gaz. Méd. de Paris*, November, 1871.

17. The author is, on the whole, favorable to intra-uterine injections in certain cases. He considers the supposed danger of the passage of the injections through the Fallopian tubes into the peritoneal cavity as chimerical; and he believes any suffering or distress they may occasion to be merely temporary and of a reflex nature. He admits the possibility of inflammatory mischief following their use, but thinks this may be obviated by a proper selection of cases, rigorously excluding those in which there is any symptom of inflammation, acute or chronic, either in the uterus or the surrounding cellular tissue. He finds them most useful in cases in which there is a morbid state of the lining membrane of the uterus, associated with menorrhagia, and in them he uses a solution of perchloride of iron.—*Gaz. des Hôp.*, November, 1871.

[The author, curiously enough, makes no reference to preliminary dilatation of the cervix before using intra-uterine injections. The unanimous opinion of all who have employed them is that this proceeding cannot be omitted with safety. Swabbing out the uterine cavity with probes wrapped in cotton wool and saturated in the solution to be used, possesses all the advantages of intra-uterine injections, and none of their risks.—W. S. P.]

II.—Pregnancy.

1. *On the Anatomy of the Human Placenta.*. By Dr. J. Braxton Hicks.
2. *Researches on the Precise Period at which the "Laminar Membrane" of the Human Placenta appears.* By Dr. Joulin.
3. *Note on the Structure of the Uterus, and the Changes which the Tissues undergo during Pregnancy amd after Parturition.* By Dr. Snow Beck.
4. *On a Case of Paralysis during Pregnancy.* By Dr. Henry Madge.
5. *On Relaxation of the Pelvic Articulations during Pregnancy.* By Dr. King.
6. *On Contractions of the Pregnant Uterus during Pregnancy, their Physiological Effects, and their Value in the Diagnosis of Pregnancy.* By Dr. J. Braxton Hicks.
7. *Contributions on the Cause of Disease of the Heart and Vessels in Child-bearing Women.* By Dr. A. H. Lebert.
8. *On Pregnancy in Connection with Heart Disease.* By M. Peter.
9. *On Irritable Bladder in the Latter Months of Pregnancy.* By Dr. W. S. Playfair.
10. *On Albuminuria and Diseases of the Bladder during the Procreative Period.* By Dr. Kaltenbach.
11. *On the Cause of the Inclination of the Pregnant Uterus to the Right.* By Dr. Guyon.
12. *On Thrombosis of the Vulva and Vagina.* By Professor Valenta.
13. *On the Prevention of Milk Abscess.* By Dr. M. Alstädter.

1. The author commences his paper by discussing the foundation of the sinus system, as first described by John Hunter. He shews how the injection he used would almost certainly produce an eruption of blood among the villi, in consequence of the extreme delicacy of the vessels ramifying on the inner or ovular surface of the decidua serotina. The paper then proceeds to describe the arrangement and condition of the vessels. The second part of the paper showed that the presence of blood amongst the villi was no argument for or against a sinus system, since it might readily come from lacerations of the decidua serotina. The author argued that if no blood were found among the villi on examining the placenta *in situ*, it might be taken as a conclusive proof against the existence of a sinus system. Dissections proving his views were given, and further arguments were drawn from the facts observed in hæmorrhage and in extra-uterine fœtation.—*Obstet. Soc.*, April, 1872.

2. In 1866 the author presented to the Academy of Medicine a memoir in which he endeavoured to show that the chorion at term did not, as is generally described, cover the fœtal surface of the placenta, but that it was replaced by a membrane of new formation called the "laminar membrane." The object of the present communication is to show the precise period at which the chorion disappears, when new membrane takes its place. The conclusion he arrives at is that the chorion begins to disappear from the fœtal sur-

face of the placenta about the tenth or eleventh week of utero-gestation, the new membrane being formed at first in connection with the great vessels, and gradually extending over the entire placenta. Microscopically it consists of fibres in distinct layers, and can be readily distinguished from the other membranes surrounding the fœtus.—*Gaz. Méd. de Paris*, March, 1872.

3. Dr. Snow Beck contributes a careful microscopical examination of the structure of the uterus, in which he advances views somewhat different from those generally taught. He does not admit the existence of a separate mucous membrane lining the uterus. He describes the walls of the uterus as consisting of two parts:—(*a*) The contractile tissue destined to expel anything contained in the uterine cavities, and which exists in greatest amount at the outer and middle portions, whilst towards the inner surface it is divided into smaller striæ with considerable spaces between them. (*b*) The soft tissue which lines the internal surface, where it exists alone to some thickness, and afterwards penetrates a considerable distance into the substance, lying between the striæ and bands of the contractile tissue. It is destined to form the placenta which is thrown off at the completion of parturition, together with the inner layer from the whole of the inner surface of the uterus, as decidua. But it is only the inner layer of the soft tissue which comes away, the principal portion yet remaining in the inner portion of the walls. The decidua and the formation of the placenta appear to be the natural development of the soft tissue which exists at the inner surface of the unimpregnated uterus, and at the completion of parturition it is thrown off from the surface as now useless products, and there does not appear any sufficient reason to apply special designations to particular portions of it.—*Obstetrical Transactions*, vol. xiii.

4. Dr. Madge believes the case he reports to be unique. The symptoms developed themselves in the early months of pregnancy, and consisted chiefly of complete paralysis, both of motion and sensation in both the hands and the feet. Dr. Radcliffe considered the case to be one of congestion of the gray matter of the spinal cord, and gave an unfavorable prognosis. There was no perceptible improvement under treatment, although the general health was better. Faradisation was now commenced, and perseveringly continued, with great benefit. Sensation began to return in the hands and feet just two months after using it, and, shortly afterwards, motion. Eleven months after the commencement of the illness the patient was able to walk downstairs without assistance. Shortly afterwards she was delivered of a dead fœtus, of about three months' growth, the presence of pregnancy, although at first suspected, not having been ascertained. This had to all appearance been retained in utero for many months. Dr. Madge believes that the paralysis in this case probably depended, as originally suggested by Dr. Radcliffe, on congestion of the gray matter of the spinal cord. He does not think that the favorable turn taken by the case depended on the death of the child, because in several cases of paralysis during pregnancy, reported by Churchill

(which, however, were not of the same kind), the patient recovered from the paralysis before delivery, and afterwards gave birth to a living child.—*Brit. Med. Journ.*, December 16, 1871.

5. After reviewing the views of various authorities on this subject, and discussing the reasons which have led many to doubt the fact of increase in the pelvic diameters during pregnancy from relaxation of the articulations, the author enunciates the following proposition: "Physiological enlargement of the pelvic canal, in addition to that which takes place during pregnancy, from thickening of the inter-osseous cartilages, is further increased during parturition by the muscular efforts of the female forcing the wedge-shaped sacrum downwards and backwards between the two iliac bones."

This thesis is sustained by a consideration of the anatomical relation of the bones, and the fact that in the cow before delivery a distinct descent of the sacrum may be observed. By this means, he maintains, not one but all the pelvic diameters are increased, and the natural shape of the pelvis preserved during labour.—*Amer. Journ. of Obstet.*, November, 1871.

6. The author calls attention to a sign of pregnancy hitherto almost entirely overlooked. He says, "After many years' observation, I have ascertained it to be a fact that the uterus possesses the power and habit of spontaneously contracting and relaxing from a very early period of pregnancy, as early, indeed, as it is possible to recognise the difference of consistence, that is, from about the third month. If the uterus be examined without friction or any pressure beyond that necessary for full contact of the hand continuously over a period of from five to twenty minutes, it will be noticed to become firm if relaxed at first, and more or less flaccid if it be firm at first. It is seldom that so long an interval occurs as that of twenty minutes; most frequently it occurs every five or ten minutes, sometimes even twice in five minutes." The author has relied upon this intermittent contraction of the uterus for the last six years as the principal symptom on which he has depended in the diagnosis of pregnancy. He does not undervalue the other symptoms usually relied upon, but he considers this one to be of great value in corroborating his evidence derived from other sources, and not likely to deceive.—*Obs. Tran.*, vol. xiii.

7. Dr. H. Lebert, of Breslau, in continuation of Prof. Spiegelberg's suggestive paper in a previous number of the Archiv, gives carefully reported details of three cases.

1. Acute articular rheumatism at fourth month of pregnancy, indication of mitral endocarditis, embolism of left arm and right leg. Abortion. Death from œdema of lungs. *Sec. Cadav.* Endocarditis with ulcerative degeneration of the bicuspid valves, embolism of the subclavian artery, terminal end of aorta and commencement of both iliac arteries.

2. Severe puerperal fever, well-marked dyspnœa, with numerous rhonchi, and bellows murmur in the region of the tricuspid valves. *Sec. Cadav.* Purulent inflammation of the tricuspid valves, large

embolism in the right pulmonary artery with purulent inflammation of it.

3. Abortion at the sixth month, periovaritis of left side, later on, pleurisy of left and then right side and indications of deeper mischief, swelling of the right knee and left elbow-joint. Death on twenty-fourth day. *Sec. Cadav.* Embolism of the pulmonary artery, double pleurisy, inflammation of the lower lobe of right lung, distinct thrombus in the vena cava inferior, blocking of the left renal vein, periovaritis of left side, purulent peri-uterine phlebitis, endometritis with partial hypertrophy, purulent inflammation of right knee-joint, chronic duodenal ulcer.—*Arch. f. Gyn.*, iii, 1, 1871.

8. In a clinical lecture the author points out that cardiac affections are peculiarly apt to become intensified by the occurrence of pregnancy. The explanation of this is, first, that during pregnancy the entire mass of the blood is increased in quantity, and therefore increased action of the heart is necessary to effect its propulsion; second, that more or less hypertrophy is a physiological accompaniment of pregnancy, and both these occurrences tend to aggravate previously existing disease.—*Union Méd.*, March, 1872.

9. The author directs attention to a form of irritable bladder not unfrequently met with in the latter months of pregnancy which seems to depend on a transverse position of the child. After pointing out the ease with which a transverse position can be detected and remedied by external manipulation, the author narrated three cases in which very distressing irritability of the bladder had been entirely removed by this simple manœuvre.—*Obs. Trans.*, vol. xiii.

10. The author in a long and exhaustive paper concludes that physiological albuminuria does not exist either during pregnancy or in childbed.

During pregnancy albuminuria may appear—

1. In consequence of catarrh of the bladder, such as arises sometimes from pressure on the urethra and the neck of the bladder.
2. In consequence of general catarrh of the urinary passages with pyelitis arising separately.
3. In consequence of backward pressure on the vessels of the kidney.
4. In consequence of parenchymatous disease of the kidney.

In childbed, albuminuria appears—

1. From catarrh of the bladder, induced either by irritation from catheterization, extension of inflammation from the sexual organs, &c.
2. From general catarrh of the urinary passages with pyelitis.
3. From obstruction in the vessels of the kidney, as in uncompensated valvular disease of the heart, swelling from struma, after severe pressure during the birth.
4. From textural disease of the kidney, or parenchymatous nephritis, or secondarily from compression of the ureters, as in parametritis, extension of inflammation, or metastatic inflammation, as in pyæmia.—*Arch. f. Gyn.*, iii, 1, 1871.

11. The author contends that this is not the result of the habit of lying on the right side, the using principally the right arm (Pajot), the lateral position of the placenta (Levret), the shortening of the right round ligament (Boivin), or the anatomical position of the rectum (Désormeaux); but the inclined plane that the under surface of the mesentery presents to the uterus when it rises out of the pelvis.—*Arch. de Phys.*, iii, 1, p. 75.

12. The author observed this accident only four times in more than 4500 cases of pregnancy, twice after parturition, once during pregnancy, and once during parturition. As regards treatment, the views are divided; whilst, on the one hand, the expectant, either the spontaneous absorption or the spontaneous opening, is always preferred, on the other hand an operative incision is always recommended. The author agrees with the former view.

13. Dr. M. Alstädter, of Pesth, states that where we have retention of the milk, the ducts and the terminal clustered cells become so engorged and distended that in the capillaries surrounding the acini, disturbance in the circulation, hyperæmia, stasis, and at length inflammation arises. He advises use of extractum conii.—*Wien. Med. Press.*, xii. 10—14, 1871.

III.—On Labour and the Puerperal State.

1. *On the Proper Management of Tedious Labours.* By Dr. G. Hamilton, of Falkirk.
2. *On the Hydrate of Chloral in Puerperal Convulsions.* By Dr. Swayne.
3. *On the Operation of Transfusion.* By Dr. A. H. Ringland.
 On a Case of Fatal Post-partum Hæmorrhage, in which Transfusion of Blood was practised. By Dr. W. S. Playfair.
 On a Successful Case of Transfusion. By Dr. Aveling.
4. *On a Case of Concealed Hæmorrhage before the Expulsion of the Placenta.* By Dr. J. Braxton Hicks.
5. *On Phlegmasia Dolens; its Origin and its Connection with Erysipelas and other Diseases.* By Dr. R. J. Lee.
6. *On the Occurrence of Bacteriæ (Bacterien) in the Diphtheritic Form of Puerperal Fever.*

1. Dr. Hamilton continues his paper on the management of tedious labours. He divides vertex presentations, for the purpose of forceps delivery, into six classes, regulated by the position of the ear:—(1) Face to right side, with ear to right of symphysis, at or very near symphysis, and on the left of it. (2) Face to left side, and the ear the reverse of these. For the most common situations, in which the author finds the ear has been at or very near the symphysis, with the face sometimes to one side, sometimes to the other (his second and fourth positions), and this and the other positions, he considers almost indispensable guides in the use of the forceps.

The second class of cases in which he has had to use the forceps, in which the head has been higher up, the ear near the symphysis,

and the face mostly to the right side but, when the head refuses to take the turn into the sacrum. Rotation he finds to be often easily and effectually performed by the forceps, the rest of the labour being easily concluded. Short, squat women are peculiarly liable to this sort of detention.

The third class of cases is somewhat similar, but where the face cannot be made to take the turn into the sacrum.

The author gives a lengthy discussion on—(1) the powers and operation of the forceps as an instrument; (2) the mechanism of labour in relation to this instrument; (3) the best kind and form of forceps.

With regard to the first point, the author holds views different from those usually taught. He disputes the action of the forceps as a double lever. "When we employ, in delivery, leverage power we act, properly speaking, not with a double but with a single lever, and this is effected by converting, as were, the forceps and head of the child into a single bar, the one end of the bar thus being the head stopped in its passage. By working on the other end the head may be gradually dislodged, but the power employed is mostly traction, for the leverage power exerted in traction is remarkably small." There is a kind of leverage which the author contends can be employed with the forceps, and which, in fact, is the highest power possessed by the instrument. This he illustrates as follows: —"If we take a large, malleable, headless nail, and drive it partially but firmly into a tree, this may be supposed to represent the head of the child. In extracting this wedge we can employ three methods —(1) Laying hold of the wedge with a pair of sharp-pointed pincers, we can extract it by simple traction; and in this way we can also deliver the child, but the power employed is not great. (2) We can extract it by employing a motion from side to side; this is more powerful than the first, but the leverage power obtained is not great. (3) We can extract by laying hold of the wedge and forcing the pincers and it to move in part of a circle round the tree. The force employed here is in great part leverage, and is much greater than either of the other two." This is the power which the author chiefly employs in cases embraced under this third class. In order to use them thus, however, it is necessary to use straight forceps, and to place them antero-posteriorly in regard to the pelvis.

The instrument used is Dr. Hamilton's modification of Ziegler's forceps, a very powerful instrument, with a fenestrum continued down to the lock in one blade, through which the other is passed, so that the second blade must lock. The double-curved forceps Dr. Hamilton never uses. The method of rotation here described is detailed at full length, and can hardly be made very intelligible in a short abstract. The author thinks so highly of it that he believes podalic version in cases of slight pelvic contraction should never be used as a substitute for the forceps until it has had a fair trial.—*Med.-Chir. Rev.*, January, 1872.

2. Dr. Swayne relates two cases of puerperal convulsions in which

hydrate of chloral was given after venesection. The author's experience leads him to the conclusion that the action of chloral in this disease is similar to that of chloroform, but that it is more steady and persistent in it effects, and that it is much more manageable, because we can regulate the dose with much greater certainty. He believes, however, that it should not supersede bleeding, in the efficacy of which he has great confidence, but be used in conjunction with it. He believes that the dose required to produce a decided effect must be large—at least forty grains at a time—and that it may be repeated in three or four hours.—*Brit. Med. Journal*, December 30th, 1871.

[The value of chloral as a remedy in puerperal convulsions can hardly be doubted. A word of caution, however, may not be out of place with regard to the large doses of the drug frequently administered. The reporter has seen one case in which the doses recommended by Dr. Swayne were given, with the effect of controlling the convulsions, but excessive depression followed, from which the patient never rallied. Doubtless the susceptibility to the action of the medicine varies in different cases, and its effects will require to be very carefully watched when administered in such large quantities.—W. S. P.]

3. Dr. Ringland relates the particulars of two cases in which transfusion of blood was practised, in one unsuccessfully, in the other successfully. In both defibrinated blood was used, and the simple apparatus devised by Dr. McDonnell, in which the blood is placed in a pipette, and the propulsion effected by atmospheric pressure only.

In the discussion which followed Dr. McDonnell made some interesting remarks. He dwelt on the advisability of using defibrinated blood, thus not only getting rid of a useless and excrementitious ingredient, but avoiding all fuss and allowing the operation to be performed slowly. He was of opinion that the risk of trying to inject blood which was not defibrinated was very great, and rendered the patient more liable to death from embolism. Thus, physiologically, defibrination of the blood made it better for the patient, and surgically it disarmed the operation of most of its dangers and risks. With regard to the difficulty experienced in Dr. Ringland's second case in getting the blood to flow, he attributed it to being too precipitate, and he believed that gravitation alone, or aided by the pressure of the mouth, was always sufficient to effect the transfusion.—*Medical Press and Circular*, January 17th.

Dr. Playfair relates a case in which transfusion was practised when the patient was apparently moribund; defibrinated blood was used. The gravitation apparatus was first tried, but found not to act, and the operation was completed with an ordinary syringe. The patient rallied, and lived for three hours after the operation. Unfortunately, a second gush of hæmorrhage occurred, although the uterus had been freely injected with the solution of perchloride of iron, and this proved fatal.—*Lancet*, January 27th.

(Dr. McDonnell's apparatus is so simple and inexpensive that it is

much to be regretted that its transfusing power should be a matter of doubt.

In one of Dr. Ringland's cases very great difficulty was experienced, and in the reporter's it would not act at all, although mouth pressure was freely employed, and plenty of time given. It certainly was not abandoned from precipitancy, but because after careful trial it was found not to work. The importance of using defibrinated blood can hardly be over-estimated, as it renders the operation so much easier and safer, and it acted admirably in the reporter's case.—W. S. P.)

Dr. Aveling relates a successful case in which his plan of mediate transfusion was employed. The details of this case are not yet published, but his apparatus seems to have acted admirably.—*Obst. Soc.*, April, 1872.

4. Dr. Hicks calls attention to certain cases in which a portion of detached membrane, after the expulsion of the child, is pushed down over the os, behind which blood collects.

The examining finger detects a tense bag, which may be mistaken for that of a second fœtus. Generally, the membranes remain attached to the lower portion of the uterus, while the part of them towards the fundus becomes, along with a portion of the placenta, separated by the effusion of blood, which gradually pushes it down towards and through the os uteri.

The treatment is obviously to puncture the bag of membranes, while the uterus is compressed by the other hand. As the blood escapes, the usual means for ensuring contraction of the uterus are to be adopted.—*Brit. Med. Journ.*, February 24th, 1872.

5. The author relates a case of phlegmasia dolens which attacked a female who had been exposed to the contagion of erysipelas. The disease showed itself first in the right leg, and then in the left; at the end of three weeks she was making favorable progress, when cardiac symptoms came on suddenly, and rapidly proved fatal, probably through pulmonary embolism.

The author believes that the coagulation of blood in the veins is due to inflammatory changes in the connective tissue surrounding the vessels, which he believes to precede and cause the coagulation.—*Lancet*, April 13th.

[It is to be regretted that the history of the case and the post-mortem examination was not given in detail, as well as of the other cases on which Dr. Lee founds his views. His theory leaves entirely unexplained the cases of sudden death from thrombosis of the pulmonary arteries, in which the clot forms *in situ*, the existence of which can hardly be denied, and which evidently depend upon blood changes similar to those producing phlegmasia dolens.

Clearly the formation of the coagulum in these cases cannot depend on perivascular cellulitis.—W. S. P.]

6. Professor Waldeyer, of Breslau, records that he has found these in great numbers in the diphtheritic deposit on the inner surface of the uterus the puriform mass from the lymph-vessels of the

uterus and broad ligaments, the peritoneal exudation, both in the fluid and also in the whitish-yellow purulent fibrinous flakes. Haussmann formerly only found the Bacteriæ and vibriones in the lochial secretion. In the diphtheritic exudation these Bacteriæ lie between the pus-cells and those of the partly decomposed tissue elements. The puriform contents of the lymph-vessels consist of, besides the pus-cells, for the most part of Bacteriæ, and they are also found in the peritoneal exudations and flakes, mixed with pus-cells, young cells, and masses of fibrine, and even in the interior of the pus-cells. The change begins in the lining surface of the genital canal, creeps through the lymph tracts up to the serous membrane; then develops itself, generally arising from a pelviperitonitis, a purulent exudative inflammation of the abdominal walls, which rapidly extends to the diaphragm, and even to the pleura, pericardium, and the joints.—*Arch. f. Gynk.*, iii, 2, 1872.

IV.—The Diseases of Children.

1. *The Normal Temperature of Infants.* By Dr. C. Vibz.
2. *On Erysipelas in Early Infancy.* By Professor H. J. Abelin.
3. *On Thoracentesis by Suction in Empyema and Hydro-pneumothorax.* By Dr. Bouchut.
4. *On the Treatment of Empyema in Children.* By Dr. W. S. Playfair.
5. *On Incontinence of Urine in Children.* By Dr. J. Warrington Hayward.
6. *On Infantile Paralysis.* By Dr. Benjamin Ball.
7. *Anatomical and Physiological Researches on the Spinal Paralysis of Infants.* By MM. Henri Roger and Damaschino.
8. *On Spina Bifida successfully treated by Tapping and Pressure.* By Dr. Cabana Cabral.
9. *Tænia in a New-born Infant.* By Dr. Armor.
10. *Therapeutics of Croup.* By Dr. Bouchut.
11. *Observations on the Frequency and Symptoms of Rachitis.* By Dr. J. S. Parry.
12. *Ovarian Tumour in an Infant: Operation at the Age of Three Years.* By M. Alcock.
13. *On Trismus Nascentium.* By Dr. Bailey.
14. *Epileptiform Convulsions produced Experimentally in an Anæmic Child.* By Dr. Eminghaus.
15. *On the Diagnosis of Phlebitis and Thrombosis of the Cavernous Sinuses of the Dura Mater in Children.* By Dr. Renier.
16. *On the Diagnosis of Chronic Hydrocephalus by the Ophthalmoscope.* By Dr. Bouchut.

1. Dr. C. Vibz, of Stettin, shows that the temperature in the rectum of infants before the cord is severed is higher than in the maternal vagina, but it sinks rapidly during the first two hours after birth, then rises, and between the thirtieth and thirty-sixth hour reaches its maximum (37·5—37·9° C.°).

From this it gradually falls till about the fourth day, and then rises again until about the eighth. Warm baths increase, and sleep depresses the temperature. From fifty observations, he states that the temperature rises from the first hours of the morning through the forenoon, about midday or the early afternoon reaches its highest point, and then again begins to sink in the evening.—*Jahr. f. Kinderheilkund*, N. F. iv, 4, 1871.

2. Professor H. J. Abelin narrates some cases. He recognises three forms of erysipelas:

The *first* in children shortly after the birth; the most severe of all, and seeming to depend upon some blood poisoning, as in puerperal diseases.

The *second* occurs in children from two months to one year old, traumatic erysipelas, the mildest of the three forms, generally running a favorable course without serious symptoms, and limited to the affected part.

The *third*, the so-called wandering (wandernde) erysipelas, appears without any indication of internal or external injury, and has a tendency to spread rapidly over the whole surface of the body, or greater part of it, often advancing by irregular leaps, and not continuously.

Ordinary treatment has no effect. The most efficacious mode of dealing with it is by means of warm baths at a temperature from 100 to 108° F. He noticed a rapid diminution of tension and heat of the skin, cessation of pain, and if employed early enough, they limited the extension. He advises immersion from fifteen to thirty minutes, and then to swathe the infant in a warm blanket for an hour or two. In severe cases the baths should be repeated twice daily.—*Nord. Med. Arch.*, II, 9, 1871.

3. The object of Bouchut's paper is to prove the superior advantages of aspiration in the treatment of empyema. He commences by an historical *résumé* of the operation, and describes the various methods used in giving exit to the pus, including the old Galenic plan of simply opening up the cavity of the chest by an incision in the intercostal space, and its more recent modification, in which a Chassaignac's drainage tube was introduced.

He then proceeds to describe his own plan, which consists of frequently repeated aspirations with Dieulafoy's pneumatic aspirator, often repeated two or three times in the week. He believes this method to be far superior to any other, but points out that if the lining is bound down by adhesion, it will not expand to take the place of the fluid removed, and that then it may be necessary to make a counter opening, and introduce a drainage tube. This method, however, evidently involves a protracted treatment, for if in the three cases brought forward by the author as illustrative of the advantages of his method, one was still under treatment after nine months' continuous operation, another was discharged cured after six

months' treatment, and thirty-nine operations, while the third case ended fatally.—*Gazette des Hôp.*, January, 1872.

4. The author pointed out that the operation of paracentesis in serous pleurisy and in empyema stands on a different footing. In the former we only require to remove a portion of the fluid in order to facilitate the absorption of the remainder. In the latter, the chances of absorption being reduced to a minimum, the complete drainage of the pleural cavity is of great importance. The author believes that the exclusion of air is of great importance in lessening the duration of the cure, and in preventing subsequent chest deformity. He describes a method of continuous subaqueous drainage, which was employed with marked success in three cases. It consists essentially in leaving one extremity of a fine drainage tube in the pleural cavity. Certain precautions are taken to prevent the entrance of air at its point of penetration, the other end being placed in a bottle of water under the bed. Time is thus given for the gradual expansion of the lung, and a gradually decreasing amount of pus is daily forced through the tube into the bottle, until at last the pleural cavity is quite emptied.

Of the three cases narrated one was perfectly convalescent in seven days, the other two in thirteen and twenty-eight days respectively. These results contrast very favorably with those arrived at by the method of repeated aspiration recommended by Dr. Bouchut in his article already alluded to.—*Obstet. Soc.*, January, 1872.

5. The author divides cases of this affection in—(*a*) those depending on deficient nervous or muscular action; (*b*) those which have for their cause an excess of this action; (*c*) a small number depend upon indolence or bad habit.

In class depending on want of sphincter power there is incontinence by day as well as by night, and they are best remedied by tonics, especially strychnia. Cases due to the second class of cause are more common, and many of them arise from spasm, the result of some irritation, one of the most common being congenital phymosis, and these are at once cured by removing the exciting cause.

Failing this, the best treatment is by giving belladonna in gradually increasing doses until some physiological effect is produced. Chloral the author has not found to be nearly so efficient.—*Lancet*, May 11th, 1872.

6. Dr. Ball, in a clinical lecture on this subject, dwells at some length on the changes in the spinal cord. These are principally found in the central gray substance of the spinal cord, chiefly affecting the anterior cornua. In these the lesions manifest themselves by a diminution of all the diameters, and a more or less complete alteration in form of the entire cornua. In certain spots attentive examination shows that extensive groups of nerve-cells have disappeared—sometimes all the cells of a particular region—their place being occupied by a transparent filamentous substance, traversed, at

intervals, by delicate nerve-fibres. The white substance is much less materially altered, and that in the anterior columns only which is often sensibly atrophied. These changes are probably inflammatory in their nature. There is a distinct relation between the local paralysis and the alterations in the gray substance of the anterior column.—*Gaz. des Hopitaux*, February, 1872.

7. By the term "spinal paralysis" the author means the well-known infantile disease which has received so many different names, in accordance with the varying views entertained of its nature, such as "essential paralysis of infancy," "atrophic fatty paralysis, with fatty degeneration of muscles," &c. They maintain that the true pathology of the disease points not to a localised muscular condition, as maintained by Bouchut, but rather to a congestive and irritative process going on in the spinal cord, to which the old term *myelitis* would not be inapplicable.

In support of this view they relate the details of all the cases in which a post-mortem examination of the spinal cord has been attained, as well as some carefully recorded cases of their own.

The conclusions arrived at are that at the point of the spinal cord, where most change is noticeable, corresponding to the source of nerve-supply of the muscles most paralysed, the same morbid alterations may always be observed, consisting of softening of the anterior cornua of the gray substance, combined with atrophy of its cells. This atrophy is most marked near the softened portions, but traces of it can also be made out in other parts of the spinal cord. In the antero-lateral columns a marked atrophy of the nerve-tubes is also noticeable.

In considering the true nature of infantile paralysis they first discuss M. Bouchut's theory that it is a disease of the muscles themselves, the alterations in the spinal cord being only secondary. This they reject on the ground of the muscular phenomena not being the first observed symptoms of the disease, and of their only becoming fully developed at a comparatively advanced period. Admitting that the true secret of the disease lies in the cord, they contend that its precise nature has been misunderstood from too much importance having been attached to the atrophy of the nerve-cells, which, they say, is never found of itself, but always associated with a lesion of the gray substance, and of the vessels, and always most distinctly when this is most extensive.

After considering the arguments against these changes being the results either of an extravasation of blood, or of softening from vascular lesion, they arrive at the conclusion that they are best explained by the supposition of their being caused by a myelitis, especially of the gray matter of the anterior columns, which alone would perfectly account for the alterations found on examination, and is in perfect harmony with the symptomatology of the disease.

The views of the author are finally summed up as follows:

(1) The characteristic alteration in infantile paralysis is a lesion of the spinal cord, of which the atrophy of the muscles and nerves is the result.

(2) This lesion is chiefly situated in the anterior portion of the gray matter of the cord, when it may be recognised in the patches of softening discovered on post-mortem examination.

(3) This softening is inflammatory, and the disease is a myelitis.

(4) The proper name for the disease is, therefore, "spinal paralysis of infancy," and nosologically it should be classed amongst the diseases of the spinal cord.—*Gazette Médicale de Paris*, Nov. and Dec., 1871.

8. The author relates a case of congenital spina-bifida which he had successfully treated. The patient was a child twenty-five days old. The tumour was in the lumbo-sacral region, and was 40 centimètres in circumference, 17 in vertical, and 10 in transverse diameter, and 6 in depth. It fluctuated, and was transparent by transmitted light. Pressure on it did not produce any convulsions, neither was there any paralysis, or other symptoms of lesion of the spinal cord. The tumour was, therefore, diagnosed to be solely a hernia of the meninges filled with fluid. It was tapped with a Dieulafoy's trocar, and 400 grammes of a transparent yellow fluid were removed. Compression was then employed by means of adhesive plaster. The operation was repeated five times. The child eventually made a complete recovery.—*O Correio Medico de Lisboa*, March, 1872.

9. Dr. Armor describes a case of tænia which occurred in a child five days old. The infant was attacked with trismus, and the same day began to pass sections of tapeworm. Oil of turpentine and oil of male fern were administered, and the child recovered after passing many joints. The mother showed no symptoms of tænia, but after appropriate treatment passed seventy segments of worms. Dr. Armor naturally expresses much surprise as to how the tænia found its way into the intestines of the fœtus in utero.—*New York Med. Journ.*, Dec., 1871.

10. The author believes that tartar emetic may be safely employed to produce vomiting in croup. In order that it may produce its action rapidly, he advises that no drink should be given during its administration. He believes it to be a most efficacious remedy in true croup, recently too much neglected, and he cites a case of great severity in which a large layer of false membrane was coughed up immediately after the patient was got under its influence, and in which a perfect cure followed.—*Gaz. des Hopitaux*, Dec., 1871.

11. The author contributes an elaborate paper on this disease as it occurs in America. He entirely controverts the usual opinion, that it occurs much less frequently there than in Europe. On the contrary, he says that at least 28 per cent. of all the children between one month and five years old coming under his care in the Children's Hospital, in Philadelphia, were rickety. The contrary opinion he attributes to the non-recognition of the earlier signs of the disease;

he has never seen a congenital case, and believes that it is rare for it to begin after the end of the first year. The general symptoms of the disease and the deformities it produces are dwelt on at length, and are identical with those in this country. The author devotes especial attention to the presence of cranio-tabes, the frequency of which has been overlooked. It was met with in about 40 per cent. of the cases he observed. Cranio-tabes does not affect the whole of the cranium, but only the posterior portion, and in all, except rare cases, only the occipital bones. The number of perforations varies greatly, there may be but one or two, or there may be many; the perforations are preceded by thickening and softening of the bone, and seems to be produced by pressure from the brain on one side and counter-pressure from the pillow on the other, and are not, therefore, as some think, the result of deficient ossification. The author maintains that laryngismus stridulus is almost invariably associated with this state. Out of the ten last cases he has seen all were rachitic, and nine had marked cranio-tabes. Hence the importance of treating the cachexia rather than the concurrent spasm.—*Amer. Jour. of Med. Science,* Jan., 1872.

12. Mr. Alcock relates an unique case of ovariotomy performed in an infant of three years of age. The patient was first seen at the age of two years, presenting an abdominal enlargement obviously due to the presence of fluid. The increase of size had been noticed for six or seven months; the true nature of the case was discovered when an attempt was made to remove the fluid, supposed to be ascitic, by tapping. A pint of straw-coloured fluid was then removed, and the existence of a cyst ascertained. A fortnight later the tumour was removed; it was found to be universally adherent. The patient died on the second day after operation with symptoms of exhaustion. The tumour seems to have been an ordinary multilocular cyst of left ovary.—*Lancet,* Dec. 16th.

13. The author contributes an elaborate essay on the subject in which he discusses the entire history and pathology of this disease, especially as it occurs among the negro population of the Southern States.

His experience leads him to conclude that it generally commences during the first week after birth, and very rarely after the second.

He believes that generally a variety of causes are in operation, and fully admits the important influence of defective ventilation, and cleanliness, bad health of the mother, and the other causes to which the disease is generally attributed as predisposing to its occurrence; but he believes the chief exciting cause to be a morbid condition of the umbilicus. In his experience he has never seen a case in which this was absent; this is generally produced in negro children by improper dressings and want of cleanliness.

He does not believe that race has any influence in the production of the disease. Its greater frequency among the negroes of the Southern States is explicable from the larger number of negro

children, and their more constant exposure to both the predisposing and exciting causes. In an experience of eighteen years in the South he never saw a well-marked case recover.

Death generally occurs from exhaustion, in a few cases from apnœa during a paroxsym.

When the disease is developed he has found no treatment of any avail, but he has often seen impending attacks averted by soothing applications to the umbilicus.—*Amer. Journ. of Obstet.*, Nov., 1871.

14. Dr. H. Eminghaus of Jena, narrates an instance of a boy, æt. 14, in whom convulsions were produced by any violent exercise, such as running; the features were rapidly distorted, the arms alternately stretched and again bent, frequent shivering of the whole body, the tongue was rolled about in the mouth, and was in danger of being bitten. The sensibility to painful pressure was arrested, the face dark reddish-blue, the eyeballs rolling prominently under the now widely extended and now closed eyelids; the pupil motionless, dilated, the heart's action increased, the accidental murmurs (systolic, mitral and tricuspid) very loud; the loss of consciousness was complete, the fit lasted three minutes, no comatose condition after the paroxysm was observed.—*Jahr. f. Kinderheilk.*, N. F., iv, 4, 1871.

15. Dr. Renier, of St. Petersburg, relates the particulars of a case occurring in a child, æt. 5 years, twenty days after the appearance of scarlatina, otitis, with implication of the meninges, severe pain in the right side of the head, somnolence, sudden shrieking during sleep, and high temperature 41·60° C. (107° F.). Paralysis of the facial, ptosis of the right upper eyelid, fixed external strabismus, dilated pupil on right side, followed by loss of consciousness and paralysis, with subsequent restlessness and furious delirium and œdema of right eyelid. On section, the right transverse sinus was changed into a thick, dark blue tumour, which on being opened contained a thrombus completely blocking up the sinus. Caries of the inflamed labyrinth was commencing.—*Jahr. für Kinderheilk.*, N. F., iv, 4. 1871.

16. The author says that the differential diagnosis of hydrocephalus from rachitic enlargement of the cranium may be easily made by the ophthalmoscope. The alterations observed in the former disease consist chiefly of venous dilatations in the retina, the result of impeded circulation in the meningeal veins and sinuses, and of more or less complete atrophy of the optic nerve, which are never observed in rickets.—*Gaz. des Hôpitaux*, April, 1872.

REPORT ON SURGERY.

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In the present report we have selected a few of the more recent improvements tending to advance the progress of surgery.

I. First of all we shall take the subject of the *healing of ulcers* and wounds by *transplantation* or *skin-grafting*. We cannot quite agree with Mr. Anderson in his remarks on the "Theory and Practice of Epidermic Grafting" ('St. Thomas's Hospital Reports,' 1871, vol. ii, p. 165), when he states that "during the past year (1871) the matter has been allowed to languish, and, notwithstanding the advantages of the practice, it is now but rarely brought under observation, nor have any additions of moment been recently made to our experience. Numerous subsequent failures have damped the ardour of followers in the footsteps of Reverdin and Pollock; the practice has been threatened with neglect," &c. This assertion is not strictly correct, for the procedure has been vigorously carried on in France, Germany, Russia, America, &c., as well as in our London and provincial hospitals, although there may have been but few recent allusions to the subject in the journals. However, Mr. Anderson gives us an exceedingly interesting account of its theory and practice. Since then M. Reverdin has commenced a series of communications, "De la Greffe épidermique," in the 'Archives Générales de Médecine' for March and May, 1872; and Mr. Bryant, in the 'Guy's Hospital Reports' for 1872, p. 237, writes an article on skin-grafting. In discussing this subject we shall take the more recent memoirs for our text, combining, as these do, all the chief points that have previously been written thereon. We shall more especially allude to Reverdin, who is generally regarded as the originator or discoverer of this grafting process.

M. Reverdin prefers retaining the original term, "epidermic grafting," although, as he says, that practically it is not quite exact, for the piece transplanted is composed of the entire epidermis and a portion of the dermis, but that it is not the latter which is the active agent; it is solely the epidermis which determines the cicatricial islets, consisting of epidermis only. Although the epidermis alone may be transplanted, yet it is well to include a small portion of the dermis, to ensure success; but it should not be the entire thickness of the dermis, as this often leads to failure. Thus, it is a term preferable to that of skin-grafting or transplantation. This latter term must not be confounded with rhinoplasty, where the transplanted structure consists of a large portion of skin and subjacent tissue, whose vitality is dependent on the neck or stem left attached to the parent stock.

The conditions necessary for success are thus summed up by Reverdin;—1. The wound should be covered with fleshy granula-

tions; 2, these granulations should have their organization sufficiently advanced, as shown by the tendency to cicatrization round the margins; 3, the granulations to be clean, and not covered by false membranes; 4, all topical applications, such as would modify the surface, especially alcoholic solutions, should be omitted for several days before; there should be no bleeding surface. We may, however, meet with success under adverse circumstances, viz. in tertiary and scrofulous ulcers; raw surfaces after removal of cancer and after burns; and here he does not object to Pollock's method of inserting the graft into a cut made in the granulation.

The part from which the graft is to be taken.—Reverdin selects the internal aspect of the leg, taking care to remove the hair from off it; others prefer the inner side of the arm or side of the chest, &c. In fact, the graft may be taken from almost every part where the cuticle has not been too much worn or altered in character. Of course, the patient himself ought to furnish his own material; still, in some instances it is requisite to obtain it from other sources, and here, perhaps, some discrimination should be used in selecting the person from whom it is to be taken, viz. not from those suffering from cancer, syphilis, &c. Grafts have been successfully used when taken from parts recently removed by operation, and here the same discrimination must be enforced. Zoodermic grafts have also been tried; thus, M. Létrévient, considering the pain and possible accidents incidental to cutting skin-grafts from the human subject, has employed grafts cut from the skin of the belly of a dog, in a place unprovided with hair. The grafts succeeded, and he recommends the process ('British Med. Journ.,' January 20th, 1872). Again, M. Coze, Surgeon to the Military Hospital of Perpignan, has submitted to the Academy of Sciences of Paris three cases of obstinate ulcers, which he succeeded in healing by grafting upon them cutaneous particles taken from the rabbit ('Lancet,' April 27th, 1872).

Respecting the size, thickness, &c., of the graft.—Reverdin says, "The graft should always comprise the living cells of the epidermis, and those of the Malpighian layer, and hence necessarily involving the papillæ which ensheath them. The dermis plays only a secondary *rôle*, so that a thick well-nourished flap is not required; indeed, it is generally difficult to adhere, and often fails. The smaller the graft, the more liable is it to take, and Reverdin prefers one of three millimètres square. The number of grafts to be used must depend upon the size of the ulcer. The pieces may be taken individually from the spot selected, or a fine strip may be removed, and then carefully divided into minute portions, avoiding injury or bruising of the epidermic cells. Some have used the skin-dust, but, unless it contains vivifying cells, it is worse than useless.

Mode of grafting, and instruments required.—M. Reverdin prefers his own method to all others. He takes the graft from the inner side of the leg, and with the thumb and index finger he stretches the skin over the plane surface of the tibia; then introduces the point of a lancet, parallel to the bone, at a very slight depth, of about half

a millimètre, pushing it across in the same parallel direction to the extent of three to four millimètres, thus removing a small piece on the lancet. The lancet thus charged, the graft is made to slide on to the granulating surface by means of a pin, taking care that the graft does not roll up. After completing the grafts he covers them with diachylon, which is not to be removed for twenty-four hours; indeed, he prefers leaving inspection for three or four days.

Pollock ('Clin. Soc. Trans.,' vol. iv) says, "I have usually removed the skin by nipping up a very small portion with a fine pair of forceps, and cutting it off close with sharp scissors. At first I made a slight cut in the surface of the granulation, and then imbedded a piece of skin; but of late I have only laid it on the surface of the ulcer. I cannot say that I have found any difference in the result. I do not think there is great, if any, advantage to be gained by the transplantation of a large piece, but when the ulcer is large I think much is gained by the transplantation of numerous small pieces."

A special pair of scissors for grafting purposes has been used by Mr. Bryant, a figure of which accompanies his paper in the 'Guy's Hospital Reports;' it is made after a suggestion of Macleod. It is exactly similar to the pair described by Mr. Cripps in the 'Lancet' for June 3rd, 1871, to facilitate skin-grafting. It consists of a pair of curved scissors, which are provided on their concave surface with bent forceps. They are controlled by a lever which descends on the separation of the blades, and rises when they are brought together. The movements of these several parts are so concerted that the forceps meets between and just below the blades, immediately before the closure of the latter, and then rises between them to such a height that whatever they have seized will be divided from its attachment when the blades meet. Thus the whole process of seizing a small portion of skin, separating and raising it, can be almost simultaneously performed with one hand. The size of the severed piece of skin is proportionate to the force with which the forceps are pressed against the surface from which it is to be removed.

On the formation or development of the grafts or islets.—This has been very accurately described by Reverdin, and also by Anderson in the 'St. Thomas's Hospital Reports.' Reverdin's account is the following:—At the end of twenty-four hours the graft is adherent, but may readily become detached; at this period the surface of the piece presents a more whitish aspect, seems thicker, as if swollen, is softened, and sometimes a little wrinkled. At the end of forty-eight hours it is already surrounded by a slight zone of a very narrow, pale gray colour, and not unfrequently separated from it by an excessively delicate transparent circle. In the course of three or four days there is observed on the borders of the zone a more or less larger one, presenting peculiar characters, being of a deeper red than the granulations, and is smooth, becoming more apparent on exposure to the air. Whilst this red zone is commencing to form, the piece or graft with its areola recedes below the level of the wound. On the

following day the red zone of yesterday takes on a decided gray coloration, and a new smooth red areola forms around it. Thus the process continues until a cicatricial island is formed, and by degrees the central part becomes white. Such process is repeated at every graft. Reverdin asks, "What becomes of the graft or transplanted piece?" He says sometimes it remains a long time white and prominent, at other times there is abundant desquamation, and the piece is no longer distinguishable. Its site may remain marked by a deep depression. When a graft is placed in the centre of a wound it becomes circular, and has the white, grey, and red zones perfectly circular; when placed near the margins of the wound, the islet elongates and grows towards the margin, at the same time that the marginal cicatrization extends towards it, so as to meet. A graft will extend towards the nearest marginal cicatrizing part. Reverdin has seen these grafts attain the size of a one or two franc piece, but generally they reach to the size of a twenty or twenty-five cent piece. Dobson ('Med. Times and Gaz.,' Oct. 29th, 1870), found that they never grew larger than a sixpence.

Woodman, in his pamphlet on 'Transplantation of the Skin,' asks, "How does this growth of skin take place? Does the original piece that is engrafted grow?" He answers in the negative, and adduces one of Pollock's cases of engrafting from a black person as corroborative of his assertion. He believes that the graft acts by influencing in some way the surrounding granulations, and inducing them to assume a similar form; so that this influence only extends to a certain distance. Dobson held the same views, in asserting that the act of transplantation is simply to insert a pattern of skin, and this pattern is impressed on the granulations to a certain but limited extent; it simply supplies them with a proper model whereby they may fashion themselves.

Bryant's remarkable case of grafting from a black man is quite opposed to the deductions offered by Woodman, and he conclusively shows that the engrafted portions grow by the prolification of their own cells, as maintained by Reverdin. Bryant thus records his observations:—"In the case of a white man upon whose leg four small pieces of black skin were grafted, the whole being no larger than a barley-corn, the black skin grew twenty-fold in ten weeks; these at first gradually enlarged, then sent out prolongations and united, until one patch of black skin was formed.

On the influence of the graft on the cicatrization of the margins of the ulcer.—This is undoubtedly the case, and Reverdin considers it difficult to comprehend; he thinks it may be a coincidence of a sudden change in the whole surface of the ulcer. Anderson gives a much more simple explanation of the renewed marginal activity consequent upon the influence of grafting; he says, "It may be elucidated by a more common-place light; one may produce a precisely similar phenomenon by the application of an ordinary stimulant; thus, by the fixture of a piece of sheet lead upon the centre of granulations, or after a gentle use of caustic, the action is simply an effect of the stimulus of the grafting as a process inducing a local

determination, in the same way as a foreign body or any ordinary stimulant.

Respecting *the rapidity of the cicatrization*, the process is generally very striking, and where a number of grafts have been employed the cicatrization is truly marvellous, the healing process being daily visible. We have seen an ulcer the size of the palm of the hand, with six grafts which had taken and had commenced to strike, heal up and become cicatrized over in the space of one week.

On the condition and solidity of the cicatrix formed by the grafting process.—The aspect of the cicatrix, at an early period, says Reverdin, is the following:—The centres corresponding to the pieces, always remain prominent for a certain time, and around these is a zone of variable extent, the cicatrix being more or less elevated above the surface of the surrounding parts; it is whiter and thicker, and these zones are often of the size of a twenty or fifty centime piece. Beyond these limits or zones, in the intervals of the islets, the cicatrix becomes gradually thinner, more fragile, and more exposed to excoriation. These characters, however, often in time disappear, so that the whole is involved in one cicatrix. The liability to a recurrence of ulceration occurs in those cases where there is slow marginal ulceration, and where only one or two grafts have been made. Reverdin regards the cicatricial islets due to grafts to be more solid and better able to resist destruction than the cicatrices developed without their aid.

Woodman considers that the skin which grows around each graft resembles true skin very much more closely than the lowly organized plastic matter of an ordinary cicatrix, and that it has more elasticity and more power to resist the strain put upon it afterwards, besides being less likely to contract than an ordinary cicatrix. The grafts will have a higher organization, and be, therefore, not nearly so likely to ulcerate again. The cicatrix is smooth, pale, and deficient of hair-bulbs and glands.

There is no doubt that during the early period of cicatrization the tender coating may break down and ulcerate from slight causes, and hence, as Bryant says, the necessity of observing much care in the after-treatment, in the shape of protection and support.

Reverdin next enters upon the value of epidermic grafting in certain classes of cases, and first in respect to cicatricial contractions and vicious cicatrices. Can we prevent such by employing the grafting process? He advises its use—1st. In wounds about the commissures or flexures of the limbs, by implanting them so as to oppose the soldering of the two neighbouring granulating surfaces. 2nd. On surfaces exposed to retraction, such as the face, end of stumps, region of joints, &c. 3rd. To remedy deformities already existing, as in ectropia, &c. He next advises its use in wounds which will not heal, or which heal but slowly, and instances many cases of success, and amongst others those of Pollock ('Lancet,' November, 1870), Dobson and Tait ('Med. Times and Gazette,' October, 1870), Goldie, Lawson, Mason, &c.

Although Reverdin has not completed his monograph on the

subject, still he has afforded us sufficient material to enable us to take a very fair view of the art and uses of grafting, and which we have endeavoured to represent in the foregoing extracts.

II. On the Treatment of Aneurism.—Attempts are still being made to improve the means of arresting this disease. Internal aneurisms and aneurisms at the root of the neck are now receiving special attention. The revival of the old treatment by internal remedies has been admirably discussed by Mr. Tuffnell, of Dublin, in a pamphlet entitled 'The Successful Treatment of Internal Aneurism.' The application of ligatures and compression on the distal side of the sac; the use of galvano-puncture; manipulation, &c., are other measures that have been lately resorted to, and the whole subject in connection with the treatment of thoracic, subclavian, and abdominal aneurisms has been under consideration, in a course of lectures delivered at the Royal College of Surgeons of England by Mr. Holmes, Professor of Surgery and Pathology to the College. We shall await with interest the publication of this interesting and valuable course.

As regards external aneurisms, there has not been much novelty of late; but digital compression of the artery on the cardiac side, under chloroform, has been successful in iliac and axillary aneurisms: one of the most interesting was a case under Mr Cooper Forster, of Guy's Hospital, where a large and fluid axillary aneurism was cured by digital compression of the third portion of the subclavian artery, under chloroform, in the short space of seven hours uninterruptedly continued. We are sorry to find that compression of the femoral artery for popliteal aneurisms is still the subject of failure in the London hospitals, and we cannot help thinking that there is some truth in the reasons given by Dr. Macnamara on this point. At the annual meeting of the British Medical Association, held at Plymouth in August, 1871, he made the following remarks:

"The treatment of aneurism by compression has been attended with such happy results in the hands of Irish surgeons—results contrasting in so marked a manner with the exceptionally favorable cases occurring in the practice of English surgeons—that I have, on more than one occasion, been asked by eminent London authorities how I could account for this discrepancy.

"Our Irish plan in treating a popliteal aneurism by compression is is the following:

"We first carefully ascertain the condition of the patient's general health. If anæmic or hyperæmic, we take appropriate measures; and when we are satisfied on this point, we apply some one or other of the most improved compressors—those in which the compressing power is modified by elasticity. With this we compress the artery in the upper portion of its course, having previously arranged, some three or four inches lower down, the auxiliary instrument by means of which we propose to alternate the pressure. The upper instrument is now made to command the artery, so as but just to arrest the pulsation in the sac. This is the most delicate step in all the

procedures, and is regulated by the hand of an intelligent assistant, who at once informs us when the pulsation is arrested; and then and there the further application of pressure is arrested. A roster of intelligent students is now organized, and to them is entrusted the management of the case; two are appointed to take charge of the patient for one hour, when they are relieved by two others, and so on during the day, whereby we secure unwearied attention during the period that pressure is kept up; and as in Dublin we visit our hospitals at 9 a.m., the treatment generally commences about that hour, and is continued up to 9 p.m., when all pressure is removed, and the patient is encouraged to take his night's rest undisturbed. Next morning the treatment is resumed, and so on till the cure is perfected. At the commencement of the case we take the patient into our confidence, explain to him the nature of his case, and the method we are about to adopt for his cure. The value of this procedure is very frequently demonstrated by the intelligent interest exhibited by our patients in the management of their own cases, so intelligent as in protracted cases to supplement, if not altogether to supersede, the supervision of them by our students.

"Should one compressor prove irksome, we try another; if all should fail, we have recourse to digital pressure, or to compression by means of weights; but in every instance, convinced of the soundness of the plan of treatment, we leave no stone unturned to secure its success.

"I now come to the question so often asked me: Why we should be so successful and you so much the reverse in thus treating aneurisms?

"1. Above all, we have faith in the efficacy of our treatment.

"2. The spirit of nationality amongst us to try every expedient ere acknowledging a failure,

"3. Our hospitals are visited at 9 a.m. By our arrangements we are enabled to visit our patient and to commence his treatment under our personal supervision at an early period of the day.

"4. All our hospitals are within a few minutes' drive of the residences of the surgeons, and thus we are enabled to pay repeated visits; there is no eye equal to the master's for seeing that the work be done.

"5. We are not afraid, during the night, to remove all pressure, so as to secure for our patients uninterrupted sleep, whereby we keep up their health to the condition most favorable for fibrinous deposits in the sac.

"6. That some of our success must be attributed to the intelligence of the patients.

"Finally, the physique of our patients differs widely, I suspect, from yours. I fancy the disease occurs, in the majority of English patients, in the persons of well-fed, robust, and plethoric individuals; such persons will require a compressing force of high tension; as a rule, if the force employed exceed that which would be represented by a dead pressure of nine pounds, the pain to which it will give rise will soon render it intolerable."

Mr. Bryant, in his inaugural address as President of the Medical Society of London, March 11th, 1872, offered the following propositions on aneurism to the notice of the medical profession, which we transcribe as calculated to further promote the advancement of surgery:—

Propositions on Aneurism.—1. From the pathological evidence we possess it appears that for the cure of a sacculated aneurism its mechanical closure by a clot is all that is required. The typical laminated clot only takes place when the cure is chronic, it is in no way to be considered as an essential part of the cure, although to a degree most clots are laminated.

2. From pathological evidence before us it likewise seems probable that the coagulation of the blood in an aneurismal sac may be obtained by obstruction either to the afferent or efferent vessel; that distal as well as proximal pressure are both capable of producing the same result.

3. Accepting the facts as proved, that obstruction to the afferent vessel may be secured by forced flexion of an extremity as well as by digital or instrumental pressure, clinical evidence is wanting to determine the conditions under which these forms of practice may be expected to succeed separately or combined.

4. Accepting the fact likewise that all aneurisms of the extremities cannot be successfully treated by forced flexion and compression of the afferent artery, it is desirable to know under what circumstances such a plan of treatment is inapplicable.

5. Acknowledging also the great principle of practice which most surgeons now accept, that no large vessel should be ligatured for aneurism unless the treatment by compression in one of its forms has failed or is inapplicable, it is desirable to know under what circumstances the treatment by ligature should be primarily applied.

6. Complete obstruction to the afferent vessel appears to be the more rapid mode of cure than the incomplete.

7. Digital pressure to cover the artery and mechanical pressure by a weight to save muscular force seems to be the best mode of applying pressure when it can be carried out.

8. Mechanical pressure should only be had recourse to when digital cannot be employed, the elastic pressure being probably its best form.

9. When the afferent vessel cannot be occluded by surgical means, there is reason to believe that the same coagulation in an aneurism may be secured by the occlusion of the efferent, either by pressure, ligature, or other means.

10. The tubular or fusiform aneurisms are probably cured only by some inflammatory action in the walls of the dilated vessel, the fibrinous secretion being probably of the plastic kind, bearing a certain analogy to the adhesive process in other parts, as pointed out by Luke ('Med. Times and Gaz.,' 1845). But this subject is not one upon which I propose to raise a discussion.

III. On Torsion of Arteries.—The application of torsion to

the larger arteries does not gain favour with the profession, and is probably due to the natural fear and dread of insecurity in the plan. Nunneley, in his address in 1869 on "Surgery," says, "I must say I could not twist a large artery and lie comfortably in bed the next night, lest while I slept the elastic artery would untwist itself, and my patient bleed until he slept never to wake again." This dread is now proved most incontrovertibly to be a truly false alarm. At Guy's Hospital a ligature is seldom or never used; torsion is practised with unparalleled success. We have applied torsion to the subclavian artery in amputation of the shoulder-joint, and without failure; but we should not be disposed to attempt it again, as the artery is too loose, and the twist is not confined to the lower part of the artery, the artery requiring to be drawn out and held at some distance in order to apply the torsion properly.

Lister prefers the antiseptic catgut ligature for securing vessels, and in his address on "Surgery" ('British Med. Journ.,' August 26th, 1871) he says, "Although I have used nothing but this ligature for securing vessels in wounds for more than ten years, excepting torsion, which I comparatively rarely resort to, and though in certain classes of cases putrefaction cannot be avoided, in no instance have I seen the catgut knot come away, nor have I ever known secondary hæmorrhage or abscess caused by its use."

This antiseptic catgut ligature for arteries, introduced by Professor Lister, is one of the most important additions to the progress of surgery; its use is still in its infancy, and great results are anticipated in its application to the larger trunks, where the silk ligature has failed. He thus sums up its value:—"It appears, then, that by applying a ligature of animal tissue antiseptically upon an artery, whether lightly or gently, we virtually surround it with a ring of living tissue, and strengthen the vessel where we obstruct it. The surgeon, therefore, may now tie an arterial trunk in its continuity close to a large branch, secure alike against secondary hæmorrhage and deep-seated suppuration—provided always that he has so studied the principles of the antiseptic system, and so carefully considered the details of the mode of dressing best adapted to the particular case in hand that he can feel certain of avoiding putrefaction in the wound. For my own part I should now, without hesitation, undertake ligature of the innominata, believing that it would prove a very safe procedure." "The catgut, as tied in the ordinary reef-knot with the ends cut short, seems to me to be a perfect hæmostatic. It has all the simplicity and universal applicability of the ligature, with, at the same time, the virtual absence of any foreign body from the wound."

On constriction of arteries.—Dr. Spier, surgeon to the Brooklyn City Hospital ('Medical Record,' April 1st, 1871), has invented an instrument called the *artery constrictor* as a substitute for the ligature, acupressure, and torsion. It consists of a flattened metal tube, six inches (more or less) in length, open at both ends, with a sliding steel tongue running its entire length, and having a vice arrangement at the upper extremity, by which it can be made to

protrude from or retract within the tube or sheath. The lower end of the tube is hook-shaped, so as to be adapted to the artery to be constricted. It is so shaped and grooved as to form only a compressing surface, by which means the artery, when grasped and acted upon by the force of the vice, is compelled to assume the form of the curve of the tongue: the artery, moreover, is constricted in such a way that its internal and middle coats give way, but the external coat is preserved intact. The severed internal and middle coats contract, retract, curl upon themselves, and are driven down the artery in the form of a plug by the continued pressure of the grooved tongue as it passes on into its sheath. The artery may now be slipped out of the instrument, and it will be found that the external coat has been compressed at the point where it was in contact with the instrument, and the internal and middle coats will be found severed and invaginated on either side of the constriction. This invagination is of itself sufficient to arrest the flow of blood; and as soon as the current of blood is arrested in the vessel, a coagulum forms upon the invaginated surface of the internal and middle coats, and so completes the occlusion.

The principle upon which the artery-constrictor acts is the same as that which nature follows in her efforts to restore the integrity of a divided artery and arrest hæmorrhage. It is based upon the anatomical structure and peculiar properties of the arteries, and it is, in fact, the principle upon which all successful methods for closing arteries are based.

IV. *Rattlesnake bites.*—Dr. Andersen, of Wilmington ('Amer. Journal,' April, 1872), states he has given, in addition to large doses of whiskey,—for many years employed as an antidote, though in many cases it has proved futile,—bromide of potassium in very large doses, in two cases with success; his doses being 15 grs. every hour. He believes it to be the efficient agent in Biron's vaunted antidote.

V. *Hydrophobia.*—This uncontrollable malady seems to have become more frequent during the past year, and a leading article in one of our daily papers appeared on the subject, earnestly imploring the profession to consider the subject most thoroughly and seek some antidote or measures to stay its ravages. Of course every new remedy tending towards allaying the nervous excitement is speedily had recourse to, but all such seem merely to deal with the symptoms, and not with the disease. We append the following most recent cases which have undergone treatment.

In the 'British Medical Journal' for May 4th, 1872, is an account of two cases where the hydrate of chloral was used by Mr. Ellis, of Doncaster; it was administered in doses of a teaspoonful of the syrup, diluted with a little water, every two hours, and subsequently every hour; the drug mitigated the symptoms very considerably. These two cases occurred in children who were bitten by the same dog at the same time; one was a little boy in whom the symptoms appeared three weeks after the bite; the other was a little girl, where the symptoms set in almost simultaneously with the other. Both were fatal.

Dr. George Elder records in the 'British Med. Journ.,' Dec. 9th, 1871, five cases of hydrophobia admitted into the General Hospital at Nottingham. He gives a full detail of the cases, and then makes the following remarks:

"In these cases chloral hydrate alone possessed any influence over the disease; it acted simply by deadening the nervous sensibility, thus destroying in a great measure the power of reflecting impressions, possessed by all nerve-centres. If the disease be due to blood poison, is it not possible that we may convert what has hitherto been an incurable affection into a curable one, by the administration of drugs calculated to destroy the poison or eliminate it from the system?

"The five cases occurred ten weeks after the bite, and were remarkable for their duration and the marvellous way in which the spasms were kept in check by the administration of chloral hydrate. No doubt this long duration of the case was in a great measure due to the ease with which large quantities of solids and liquids were taken. Neither was there in this case any of those severe paroxysms of muscular spasms which soon wear out the patient. From admission into the hospital until the time of his death, a period of seven days, none of the most horrible features of this frightful malady were observed."

The following case is recorded by Dr. Fothergill, of the Darlington Hospital ('British Med. Journ.', Sep. 2nd, 1871, p. 264). The chief point of interest in this case appears to be, firstly, that the patient was able to use a tube with the fluid nourishment concealed. A long flexible tube was used, the one end placed in the fluid carefully covered with a cloth, the other end the patient was induced to introduce into the mouth, and he was able to swallow, though with some effort, with great comfort to himself, whereby prolonging his life. Secondly, the length of time he lived and mode of death. He died on the twelfth day; having escaped spasms of his respiratory muscles and starvation; he died apparently from paralysis of the respiratory muscles. Half-drachm doses of bromide of potassium every four hours caused considerable excitement and had to be discontinued.

VI. *Tetanus*, of course, is always under our notice. It was the subject of a very lengthy discussion at the meetings of the Société Impériale de Chirurgie de Paris, held in March and April, 1870, on the consideration of the use of chloral. Billroth's views were brought under notice; these appeared in the 'Wiener Med. Presse,' and quoted in the 'Jahresbericht f. Gesammt. Med.,' 1870, Bd. ii, Abth. 2, p. 291. In his clinical remarks he observed that tetanus seldom occurs alone, more frequently in groups, and then there were grounds for considering it a blood disease, as is hydrophobia, to which there is great analogy. In the severe rigors of intermittent fever the rigors are generally attended with spasm of the muscles of mastication and back, as occurs in tetanus; the spontaneous origin of tetanus in some cases, without any wound, is another point in favour of this opinion. Billroth finds no proof, either by pathological observation (increased

vascularity in the spinal cord) or by clinical observation, that tetanus has its origin in a neuritis ascendens and spinal meningitis. He believes it to be a poison, and further to corroborate his views he proposes to make experiments on dogs with injections of blood taken from tetanic patients. M. Després contended that tetanus was a kind of septicæmia.[1] However, the conclusions arrived at by the society seemed to be those enunciated by M. Brown-Séquard, viz. that it is the result of augmentation of the reflex faculty of the spinal cord, occasioned by peripheral excitation or irritation. Brown-Séquard considered that its ætiological history was against the poison hypothesis, more especially as concerns idiopathic tetanus; and that the development and progress of the disease were contrary to the theory; the symptoms, the pathological anatomy also, and, lastly, the treatment, seemed all to argue against its being a blood poison. Here the matter rests even now, and so long as we cannot duly explain the cause we can only treat and allay the symptoms as they appear.

A few recent additions have been made to the long list of vaunted remedies, and which have proved successful in exceptional cases—the subcutaneous injections of conia, nicotine, morphia, and the internal use of hydrate of chloral, bromide of ammonium. M. Demarquay advocates injections into the deep muscles, more especially the masseter and the muscles of the neck.

Dr. Ogle has paid special attention to tetanus, as evidenced in his collection of cases which appeared in the 42nd and 43rd volumes of the 'British and Foreign Medical Review;' and recently, in the 4th volume of the 'Clinical Society's Transactions,' he records a successful case where he applied ice to the spine, and administered internally belladonna, chloral, wine and good diet; the case was acute traumatic tetanus in a boy. Dr. Ogle's views on the subject of treatment corroborate those expressed in the article on tetanus in the 1st volume of Holmes's 'System of Surgery'; he endeavours to keep up strength, ensure sleep and repose, and diminish as much as possible the reflex excitability of the spinal cord. He placed his patient in a separate room, and apart from the noise and disturbance of an ordinary ward.

VII. *Du diagnostic et de l'extraction des projectiles.* Par Dr. Melliot, in 'Archives Générales de Médecine,' Fevrier, 1872.—*On the instruments designed for exploring gunshot wounds.* By Professor Longmore ('British Medical Journal,' December 23rd and 30th, 1871.—Both these monographs give us excellent instruction and advice on the subject. Dr. Melliot, after enumerating the several methods of exploration, calls our special attention to an electric explorer invented by M. Trouvé, a modification of instruments already known to the profession. There is an accurate description and a woodcut to facilitate explanation; the great value attached to it is its capability of being portable and thus practically useful. The apparatus

[1] Suggestions that tetanus is due to a morbid condition of the blood have been alluded to in the second edition of 'Holmes's Surgery,' vol. i, p. 330, and in 'British and Foreign Med. Chir. Review,' vol xliii, p. 297.

consists of two portions:—(1) a small case of hardened caoutchouc containing the means for furnishing the electricity, viz. zinc and charcoal, and a necessary quantity of the bisulphate of mercury; (2) an electrical apparatus in the form of a small watch with an indicator, and an exploratory sound and steel stylet consisting of two wires, but isolated and terminating in two points. The sound, which may be flexible or rigid, is passed into the wound, and made to strike the supposed foreign body; the handle is removed and the stylet introduced, so that whilst the one end rests upon the substance, the other end of the stylet is connected by its two points with the appropriate parts of the apparatus so as to complete the circuit. For a more detailed account we must refer to the original article.

Professor Longmore gives us full information on this matter in diagnosis; we have selected the following points as worthy of notice:—He first adverts to the means to be adopted in the exploration of the wound, and secondly that for their extraction or the mode of removal. After giving some very judicious and practical advice on the subject of exploration by the surgeon's finger, he passes on to the enumeration of the means of detecting foreign bodies after failure by this means. Nélaton's probe is, of course, fully discussed and approved of, and is very accurately and succinctly described. The description must be referred to by the reader. This porcelain test-probe was much used during the war of the rebellion in the United States, and many favorable reports published. Professor Longmore remarks that in these porcelain test-probes direct and firm contact between the porcelain and a bullet is essential, in order that the former may furnish the evidence which the surgeon requires. If any resisting medium, however thin—the thinnest membrane, for example—happen to be placed between the surface of the metal and the china, no impression will be made on the latter. This may lead the surgeon to an erroneous impression that no foreign body is lodged, and hence cause delay in the healing process.

Professor Longmore, in continuing this subject, passes in review the several methods that have most recently been adopted:—The instrument invented by Dr. Lecomte, a surgeon in the French army, who gave it the name of "stylet-pince" (probe nippers). His design was not merely to indicate the presence of a leaden bullet, by inducing a stain or mark of its presence, but by bringing away a small portion of the lead itself. Such an instrument could not only be used for bringing away a scale of lead, but also a minute portion of paper, clothes, wood, or fragments of bone.

The stylet-pince consists of two portions: the first is a central steel rod, of small diameter, fixed in an ivory handle at one extremity, and cleft at the other into two small branches, each of which terminates in a little cup-like blade or curette; the second is a slender canula, which glides backwards and forwards, but only within a limited distance along this rod.

The two steel curettes have very fine and sharp edges; they separate from each other by the elasticity of the two little steel branches,

of which they are the terminations; but they are easily brought together by a slight pressure, such as that exerted by causing the canula to glide along the central stem up to them. When they (the two curettes) are thus brought together to fit one to the other, they thus united form a small smooth steel knob or rounded extremity, about one third of the usual size of the china knob of a Nélaton probe.

There is no difficulty in the manipulation of the instrument. It is a most useful extractor for deciding doubtful cases of lodgment of foreign bodies; it responds as an indicator with even more distinctness than the Nélaton probe in all cases in which that tool would be of service, while it answers for a variety of other cases in which the Nélaton probe would give no indication at all.

Electricity as a means of diagnosis.—One of the most recent inventions by Mr. De Wilde, a civil engineer—consists of an apparatus very compactly arranged in a box of small dimensions. The electric action is excited in a suitable cell; the electricity there developed is increased in intensity by the intervention of a multiplying coil; an exploring probe is connected by insulated wires with the apparatus, and the indications, when the circuit is completed by contact of the two points of the probe with a leaden ball or piece of iron, are given by the striking of a hammer against an alarum bell; the bell sounds at each interruption and renewal of contact of the points with the metal. The exploring probe consists of a long slender tube of smooth vulcanite, containing two insulated needles, the points of which can be drawn within the tube or be made to protrude at the pleasure of the operator.

Messrs. Krohne and Seseman, of London, have also contrived an instrument. The indications of contact with a lodged bullet or other metal are afforded by the means of a galvanometer and of a fine needle working upon a dial-plate, in the same manner as is seen in the ordinary single-needle telegraph.

Longmore alludes to a rough but sufficiently effective instrument, which has been made in the following way:—The magnet of an ordinary pocket-compass, which has had some turns of wire covered with thread wound round it, as an induction coil, is employed for the electric indicator, whilst a piece of copper sheeting, bent round a small plate of zinc, but separated from it by flannel padding saturated with the usual diluted acid, forms the voltaic pile. The exploring instrument is formed by two insulated wires, bound together, but with the points left free; these parts being connected when the circuit is completed by contact with metal, the indication is given by movement of the magnet of the compass.

The Endoscope has been suggested. Dr. Fenger, of Copenhagen, in 1869, made experiments respecting its use in gun wounds. During the war he examined several wounds some weeks after infliction, and could see their interior without causing pain. It is not likely to be turned to much practical account. It is obvious that many of the above instruments are not suited for use in the field; the electric explorer is easily disarranged, and various circumstances render such instruments inapplicable. Besides we must recollect

that there must be a limit to search, and this limit must depend on the circumstances of each particular case, and must be decided by the judgment and tact of the responsible surgeon. Moreover the foreign body may become encysted and remain lodged without causing either pain or mischief for many years afterwards.

VIII.—*On Vertebrated Probes, Canulæ, and Catheters.*

Early in 1870 Dr. Squire, of New York, introduced to the notice of the profession a flexible metallic catheter for prostatic retention, the flexibility to be obtained by means of a series of perfect articulations in the vesical extremity of the instrument. A brief account of this instrument, with a woodcut, appeared in the 'Philadelphia Medical and Surgical Reporter' for November 5th, 1870. This catheter was exhibited and its mechanism explained at the New York State Medical Society in February, 1871, and cases detailed in which it had been used with much satisfaction. An account of this catheter, together with a report of seventeen cases, appeared in the 'American Journal of Medical Sciences' for October, 1871.

Immediately after the exhibition of this catheter at the Medical Society, New York, in February, 1871, Dr. Louis A. Layre exhibited a "jointed silver probe" for exploring tortuous sinuses in cases of diseased bone, &c. The mechanism of this probe was virtually the same as that of Squire's catheter. Dr. Layre published an account of this probe and the vertebrated catheters in the 'British Medical Journal' for July 22nd, 1871, with a woodcut, but without any acknowledgment of the source from which he derived the idea.

Mr. Durham, in 'Holmes's System of Surgery,' 2nd Edition, vol. ii, p. 511, has invented a tube for use in the operation of tracheotomy, and which he calls the canula with lobster-tail joint; it consists in the distal portion of the tube or canula being composed of several small pieces jointed together in the lobster-tail fashion; a woodcut accompanies the description.

REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIENE.

By Benjamin W. Richardson, M.D., F.R.S.

Oil of Turpentine as an Antidote against Poisoning by Phosphorus.—Dr. Köhler, of Halle, furnishes this quarter a most able essay on the above-named subject. Regarding the antidotal effects of the oil of turpentine in actue poisoning by phosphorus, there are, he affirms, six leading questions to be answered:

Whether oil of turpentine really is an antidote against phosphorus?

Whether every kind of the oil of turpentine is equally useful as an antidote against phosphorus?

Whether the oil of turpentine exercises its antidotal power, when applied to any part of the body whatsoever; or, if this be not the case, which is the fit place and mode of application?

What length of time may be permitted to elapse between the ingestion of the phosphorus and the administration of the turpentine, if life is to be saved?

In what dose and form should oil of turpentine be employed as an antidote against phosphorus?

What are the chemico-physiological processes whereon the antidotal effects of the oil of turpentine against phosphorus are founded?

1. The first question is answered in the affirmative. "Common commercial oxygenized turpentine *is* a remedy against phosphorus."

2. As to the *second question*, whether every kind of oil of turpentine may be used as an antidote against phosphorus, the author says the desired end can only be attained by the common commercial oil of turpentine containing oxygen.

The English oil of turpentine turns the plane of polarization toward the right, while the German, French, and Venetian diverts the plane to the left. The author of the present paper raised the question which of the different kinds of oil of turpentine, the one turning polarization to the right, or those turning it to the left, would form, in connection with phosphorus, the terebinthino-phosphorous acid; for it is on this chemical change, this production of a non-poisonous compound, that the antidotal power of oil of turpentine rests. Not having studied this matter before, he made twenty-nine comparative experiments respecting it with a great many different kinds of turpentine. Weighed amounts of phosphorus, together with weighed quantities of oil of turpentine, were heated in previously weighed small glass cucurbits for exactly fifteen minutes in a water bath of 30° to 40° C. (86° to 104° Fahr.), and allowed to get cold in the same place. Waiting just twenty-four hours, he weighed the crystallized substance on filters which had been previously weighed, squeezed these out strongly, and weighed again; extracted the filters and cucurbits with alcohol, dried the whole in the exsiccator as far as possible, and re-weighed. He thus got the per centage—

(*a*) Of the quantity of the terebinthino-phosphorous acid crystallized after twenty-four hours, and produced by the contact of the phosphorus with the oil of turpentine, during a quarter of an hour:

(*b*) And of the quantity of the phosphorus remaining undissolved, unaltered, crystallized, and adhering to the cucurbit and filter.

The greater the quantity of the resulting crystals of terebinthino-phosphorous acid, and the smaller the quantity of the undissolved phosphorus, the more useful as an antidote against phosphorus is the

species of oil of turpentine employed in the experiment to be regarded.

In the case of poisoning with phosphorus, a distinct order should be made on the prescription for the apothecary to dispense long-kept rectified oil.

It is to be remarked that, in the case of all oils that contain a large quantity of oxygen, if there be a surplus of them in proportion to the phosphorus, the product of terebinthino-phosphorous acid will after a time be unexpectedly small. The reason of this is the fact that the harmless product of the combination of phosphorus and oil of turpentine is soluble in the latter substance; the product being formed, indeed, though not crystallized, more of the oxygen is attracted and bound, and finally, agreeably to the description of Bamberger, a red-yellow resinous substance, containing, not phosphorous acid, but phosphoric acid, is separated. Now, since absorption as well as elimination of this said compound, which possesses little or no toxic effects, can but be promoted by its being in a soluble state, a surplus of the oil of turpentine, if the dose be not a poisonous one, may appear to be not only not injurious, but even desirable.

3. To answer the *third question*, whether oil of turpentine is effective as an antidote against phosphorus in whatever way it may be administered, or to whatever part of the body it may be applied, some ten experiments made on frogs and rabbits have solved the question.

Into these animals the author introduced—

(*a*) Phosphorus into the stomach, and oil of turpentine under the skin;

(*b*) Phosphorus into the rectum, and oil of turpentine under the skin;

(*c*) Phosphorus, and afterwards oil of turpentine, into the receptacle of lymph on the back of frogs.

All the animals died, and their bodies showed the characteristic pathological signs of poisoning by phosphorus.

The author's experiments show, with convincing certainty, that it is only by phosphorus coming into contact with oil of turpentine in the stomach that the innocuous combination terebinthino-phosphorous acid is produced, and that only then and in that way may oil of turpentine be termed an antidote against phosphorus.

4. The *fourth question*, how much time may elapse between the ingestion of the phosphorus and the use of the oil of turpentine, without the latter failing to exhibit its antidotal faculty, is by no means in a condition to be answered, too little clinical material being at hand, and animal tests not as yet being, nor to be expected to be, very relevant in this direction. The longest intervals at time between the poisoning and the successful application of the antidotes amounted to eleven hours. (Observation of author in the 'Klin. Wochenschrift,' 1870, No. 1.) However, the conclusion *à priori* is probably justified, that if a few hours have elapsed since the

ingestion of the phosphorus, and if fat victuals, moreover, have been eaten, and the stomach filled with them, sufficient time has been given for the absorption of the phosphorus, and the possibility of the oil of turpentine being still of use cannot be entertained. Here, however, so many individual differences, species, age, state of nutrition, disposition of the stomach, &c., will have influence, as to forbid the laying down of rules capable of general application.

5. Far easier on the whole is the answer to the *fifth question*, as to the doses and form in which the oil of turpentine should be given as an antidote against phosphorus. The comparative tests show that all the phosphorus is changed into terebinthino-phosphorous acid, if to 0·01 grm. (·15 grs.) of phosphorus, 1·0 grm. (15·5 grs.) of oil of turpentine, particularly the rectified commercial oil, be added. In other words, a hundred parts of oil of turpentine to one part of phosphorus. But for fresh or recently rectified oil, this assertion requires correction. For oil not rectified, or which has been rectified a long time, fifty parts are sufficient. In cases of poisoning, however, by phosphorus paste, which is very rich in phosphorus, it would be advisable to exceed the dose of 10 grms. (155 grs.) of oil of turpentine, which even large rabbits bear without their constitution in general being essentially disturbed, and even it might be to raise the dose of the antidote to one approaching a toxical height. As to the form in which it should be administered, gelatine capsules containing 5 grm. (7·75 grs.), seem to be preferable to all others. An emulsion is absolutely to be rejected, on account of the accompanying fat or oil, which would assist the absorption of the phosphorus (*Mialhe*).

6. Lastly, the solution of the *sixth question*, on what chemico-physiological processes are the antidotal effects of the oil of turpentine against phosphorus founded, has been sought, not by Jonas, as Bamberger incorrectly supposes, but by Peronne, and by the proof of experiments firstly made by the author.

Observing the smell of the urine of two dogs, fed with phosphorus and oil of turpentine, to be like the smell of opodeldoc or camphor, and this odour to get stronger still if the urine had been distilled, the author (not Jonas, as Bamberger states) first arrived at the right explanation of this question. Oil of turpentine with muriatic acid producing the well-known turpentine-camphor, he concluded that probably the phosphorous and phosphoric acids, generated by the higher degrees of oxidation of phosphorus, would produce with the oil of turpentine an analogically constructed species of camphor; and that the urine of the test-animals (camphor itself being excreted through the kidney) derived its specific smell from the turpentine-camphor, resulting from the derivatives of the phosphorus. Furthermore, if the production of such camphor from the contact of phosphorus and oil of turpentine be presupposed, and if this camphor could be proved not to participate in the poisonous effects of the phosphorus, but to have the same effects on the organism as all other kinds of camphor, then it would at the same time be shown how the oil of turpentine operates as an antidote against

phosphorus, and the ultimate aim of this discussion would be arrived at.

The author's object, therefore, was to find—

(1) Whether chemical combinations of phosphorus, and its products of oxidation, were known; and if so,

(2) Whether they are produced by the contact of phosphorus and oil of turpentine under *all* circumstances.

(3) Whether they show toxical influences toward the animal organism or not.

(4) Whether they pass into the urine unchanged, and are to be found therein by chemical process.

(5) Whether chemical combinations of the derivatives of phosphorus and turpentine take place within the animal body.

(6) Whether the identity of the resulting extra- and intra-corporal chemical combinations can be proved in an exact manner.

Kohler concludes his paper by saying that the antidotal action of turpentine over phosphorus is due to the formation of terebinthino-phosphorous acid. In doses of several grammes, the terebinthino-phosphorous acid is harmless to dogs, and therefore inoffensive in comparison to phosphorus; but it is not to be denied that it may possibly possess toxical properties in the same measure as other species of camphor.

Taken by the mouth, it travels unchanged through the body, and is eliminated in the urine. The urine, distilled and rendered alkaline by ammonium carbonate, contains the same substance, smelling like opodeldoc, and containing phosphorus. (*No other product of distillation of urine, under other conditions, will ever contain phosphorus.*) That substance produces exactly the same reactions as the solution of the terebinthino-phosphorous acid itself, reducing the solutions of the salts of silver and corrosive sublimate, &c. The generation of this acid also, by the contact of phosphorus and oil of turpentine in the stomach, is proved by the similar condition of the distilled contents, but the distillate has an acid reaction (not like the urine, alkaline). Further, oil of turpentine and phosphorus having met in the stomach, urine of the same quality results as if terebinthino-phosphorous acid had been given, *i. e.* a product of distillation, giving all the reactions just detailed, and containing phosphorus.

There can hardly be a doubt raised now, either as to the production of the terebinthino-phosphorous acid by the combination of oil of turpentine and phosphorus in the stomach, or as to the generation of terebinthino-phosphorous acid, inasmuch as it has comparatively little toxical properties, being the cause of the antidotal effects of the oil of turpentine in cases of acute poisoning with phosphorus.

For these reasons, also, Perronne is in error in supposing that oil of turpentine prevents the oxidation of phosphorus; on the contrary, by the assistance of the oil of turpentine, the phosphorus becomes, in fact, oxidized to phosphorous acid, and afterwards higher still. The oxygen, in this process, however, is furnished, not by the blood, but by the oil of turpentine given as the antidote.—*Transactions of*

the St. Andrew's Medical Graduates' Association. Edited by Sedgwick. London, 1872. (*In the press.*)

Belladonna as an Antagonist of Opium. Dr. Hasford Walker reports a case the treatment of which favours the practice of employing belladonna as an antidote for opium after poisoning by the last-named narcotic. On the night of July 24th, 1871, he was called to see a patient who had swallowed, inadvertently, a large quantity of a concentrated tincture of opium, the dose taken being considered equivalent to about six or eight grains of opium powder. Dr. Walker found his patient, a young girl of fifteen years of age, completely comatose, and on opening her eyelids and placing his fingers on the conjunctiva there was no sensibility whatever evinced. The pupils were very much contracted, the respiration was stertorous and difficult; the pulse was full, slow, and labouring; a dark suffusion was exhibited on the countenance, and there was almost a total absence of sensibility to external impressions.

Without delay, Dr. Walker, with some difficulty, on account of the almost paralysed condition of the muscles of deglutition, administered emetics of mustard and ipecacuanha, followed by copious draughts of warm water; he also tickled the fauces with a feather, and thus endeavoured to produce vomiting. In this endeavour he failed, and, not having a stomach-pump, he had recourse to the hypodermic injection of belladonna. He thus administered one grain and a half of the alcoholic extract of belladonna, and with an effect which he says was "magical." The pupils, from a state of extreme contraction, became dilated, and copious emesis followed in a few minutes.

As soon as the contents of the stomach were evacuated, the patient being much more prostrated, strong coffee and stimulants were administered, both hypodermically and by the mouth, and in the short space of two hours from the exhibition of the poison the patient had sufficiently recovered to recognise and speak to those about her. She suffered no inconvenience as a result of this treatment, except a slight degree of inflammation for a day or two, from the punctures made by the hypodermic syringe, and which readily yielded to the treatment by cold applications.—*American Journal of the Medical Sciences*, January, 1872.

[We have now recorded a great many cases in which after poisoning by opium the administration of belladonna has been followed by results of the most singularly successful character. We doubt, indeed, if there is any antidote, real or unreal, which has obtained so much apparent success as this of belladonna against opium. There is only one piece of evidence wanting to make the proofs in favour of the antidote perfect; we mean corroboration from experiments directly and accurately made on the inferior animals.

Hitherto experiments in the direction named have been doubtful and sometimes extremely unsatisfactory; but then the certainty of arriving at a safe conclusion is interfered with by the difficulty of finding a proper subject in the inferior animal grades for experi-

ments. Birds are indifferently affected by opium, cats are the same, and dogs will take very large doses of the narcotic without suffering from narcotisim, while rabbits and guinea-pigs are influenced indifferently by the belladonna. We are, therefore, driven to base all our knowledge on what has been observed in man, and we can now hardly avoid the conclusion that the belladonna exercises in the human subject a real antidotal influence over opium. It clearly relaxes the pupil, and it seems to relax the involuntary muscular fibre generally after it has been constricted by opium. In this action may lie all the antidotal power, the remedy being purely physiological in its method of giving relief. It sets free the vessels, it permits an active secretion, it removes constriction of the œsophagus, and allows the act of vomiting to occur, and so it enables the body to eliminate the poison that oppressed it. This we think is the most probable explanation of the antidotal action; but whatever explanation be given the practice is soundly sustained up to this time, and deserves a continued trial.—B. W. R.]

On the Antagonism between the Actions of Physostigma and Atropia.—The question of the antagonism of action between two active poisonous substances is well brought out in a report to the Royal Society of Edinburgh by Dr. Thomas Fraser. He opens his work by an historical notice of the various instances of antagonisms that have been suggested by different experimentalists, and he concludes this part by the hope that the description of the research forming the subject of his present communication, will render it obvious that the reality of a lethal antagonism may be readily and certainly established by experiment.

The physostigma used in the researches by Dr. Fraser was administered either in the form of an alcoholic extract or of the sulphate of the active principle. The atropia was administered in the form of sulphate. With a few exceptions, when dogs were used, the animals operated on were rabbits. The animals were invariably in a state of perfect health and in full digestion. The rabbits used were about three pounds in weight, and the plan adopted for the administration was as follows:—In the first place the minimum fatal dose for rabbits of the extract of physostigma and of the sulphate of physostigma employed was determined by a number of preliminary experiments, so that on the weight of the animal being ascertained it was an easy matter to be certain of the dose that could kill it. Then in those experiments in which the recovery followed the administration of a dose of atropia, given in combination with a dose of physostigma equal to or in excess of the minimum fatal dose, the animal used was killed many days afterwards, and when the effects of the two substances had completely disappeared by a dose of physostigma less than, or only equal to, that from which it had previously recovered. *Therefore when the administration of atropia prevented an otherwise fatal dose of physostigma from causing death, a perfect demonstration was obtained of the power of atropia to produce some physiological action or actions*

that counteracted some otherwise lethal action or actions of physostigma.

The administration of the substances was affected by subcutaneous injection, by which method the existence of a lethal antagonism was subjected to a more severe test than if the substances had been introduced into the stomach.

From these preliminary statements Dr. Fraser passes to describe his various and laborious experiments. It is not in the limits of this report to publish these, and we regret that, from some want of constructive art in literary composition, the learned author has made it very difficult to the reader always to follow him simply and with quick appreciation of results; but one or two facts must claim our attention.

It is shown that the minimum lethal dose for rabbits of extract of physostigma is 1·2 grain, and that of sulphate of physostigma is 0·12 grain, for every three pounds' weight of the animal.

It is shown by a direct inquiry as to the limits of antagonism when atropia is administered five minutes before physostigma, that while one two-hundredth of a grain of sulphate of atropia is a dose insufficient to prevent death, nine one-thousandths of a grain is a dose sufficiently large to do so; that any dose of sulphate of atropia ranging within the wide limits extending from the nine one-thousandths of a grain to five grains and one fifth is able to prevent the effect of this dose of physostigma; and that if the dose of sulphate of atropia amount to five grains and three tenths, the region of successful antagonism is left, and death occurs.

From another series of experiments it is shown that while three two-hundredths of a grain of sulphate of atropia is a dose too small to prevent the occurrence of death after a dose of physostigma one and half times as large as the minimum lethal dose, one-fiftieth of a grain is a dose sufficiently large to do so; that doses of sulphate of atropia ranging from one-fiftieth of a grain to four grains and one tenth are able successfully to counteract this dose of physostigma; and that death occurs when the dose of sulphate of atropia is so large as four grains and three tenths.

At the close of his paper the author contends that the conditions of the experiments and the symptoms that were observed render it certain that atropia prevents the fatal effect of a lethal dose of physostigma by so influencing the functions of certain structures as to prevent such modifications from being produced in them by physostigma as would result in death. The one substance counteracts the action of the other, and the result is a physiological antagonism so remarkable and decided that the fatal result even of three and a half times the minimum lethal dose of physostigma may be prevented by atropia. Whether the same results would occur in the human subject is left open by Dr. Fraser, very wisely, for experience, as distinct from experiment, to decide.—*Transactions of the Royal Society of Edinburgh*, vol. xxvi, 1872.

'*On Chromic Acid as a Test for Strychnia.*—Dr. John Dougall asserts that chromic acid elicits the coloured reaction in a solution

containing $\frac{1}{100000}$ of strychnia. The *modus operandi* is to put two or three minims of the strychnia fluid on a white non-porous surface; then add a few needles of chromic acid, which instantly dissolve, imparting to the liquid their characteristic tint. One or two drops of *concentrated sulphuric acid* are now added, and the play of colour is at once evolved. With stronger solutions the result is very distinct, and in any case unfailing. In applying this test the above method must be strictly adhered to, because if sulphuric acid be added to the strychnia solution before the chromic acid, the latter will not dissolve, and the result is negative. Chromic acid does not give the coloured reaction with dilute sulphuric acid.

It is well known that bichromate of potassium, so commonly used for this test, does not always succeed. Dougall has seen it fail in the hands of two professors—one a celebrated toxicologist—when attempting to show it to their class; and he is not aware that any explanation of the cause has been given, though it seems to him as follows:—It is obvious from the fact of chromic acid giving the coloured test at all, that to it alone the reaction is due when bichrome is used; that neither the potash in the latter, nor the sulphate of potash resulting from the addition of sulphuric acid, are indispensable agents in developing the reaction. Furthermore, as strong sulphuric acid is necessary, both when using bichrome and chromic acid, *per se*, it is evident that when added to the first, the result is the formation of sulphate of potassium, water, and free chromic acid; indeed, this is exactly the method by which the latter is prepared, as shown in the following equation:—$K_2O\ 2CrO_3 + H_2SO_4 = K_2SO_4 + H_2O \times 2CrO_3$. The chromic acid, being thus liberated, oxidises the strychnia, and the coloured reaction is afterwards developed by the strong sulphuric acid. But it may happen that the sulphuric acid, which might have proved strong enough for isolated chromic acid, is rendered too weak for bichrome, in consequence of being partially neutralised by the base of the latter, and further diluted by the water, which, as seen in the formula, is generated by the hydrogen of the sulphuric acid combining with the oxygen of the potash; and this is the most frequent cause of non-success of the test. With isolated chromic acid failure is, of course, impossible from such a source; and when used as described, the reaction is not only constant, but greatly more delicate. Another cause of lapsus with the bichrome, also applying in part to chromic acid, is seen in the following table of results:—

Bichrome added to solution of strychnia, then sulphuric acid added = much colour.

Sulphuric acid added to solution of strychnia, then bichrome added = considerable colour.

Sulphuric acid added to bichrome, then solution of strychnia added = almost no colour.

Chromic acid added to solution of strychnia, then sulphuric acid added = much colour.

Sulphuric acid added to solution of strychnia, then chromic acid added = a little colour.

Sulphuric acid added to chromic acid, then solution of strychnia added = almost no colour.

These results are just what might be expected. It will be seen, as with chromic acid, that when sulphuric acid is added to bichrome, or *vice versâ*, succeeded by strychnia, the reaction is almost *nil*. Doubtless, from the loose and empirical manner in which this test is usually employed, its failure is frequently due to the above cause.

Antidote to Chromic Acid.—In poisoning with chromic acid the best antidote is beef tea, raw egg, milk, or any albuminous or gelatinous substance. A very handy and efficient antidote is a solution of ordinary painter's size.—*Separate pamphlet*, Glasgow, 1872.

II. Forensic Medicine.

Criminal Abortion.—The New York Medico-Legal Society has issued a report on the subject of criminal abortion with the view of suggesting a plan for better legislation. The report is ably drawn, and contains many useful and thoughtful observations. The reporters commence by pointing out that the history of public opinion on the subject conveys a forcible illustration of the evil results of erroneous teaching. In the ancient days, they say, those who claimed to instruct the multitude not only looked leniently on fœticide, but, under certain circumstances, openly recommended it. Plato advocates it in a contingency mentioned in the 'Republic.' Aristotle declared that no child should be allowed to be born alive whose mother was more than forty, or whose father was over fifty years old. Lysias maintained that forced abortion was not homicide, because a child in utero was endued with a separate existence. The ancient laws, indeed, more circumspect than public opinion, punished artificial abortion with great rigour; but amongst the ancient peoples it was nevertheless largely practised under the belief that the fœtus not being alive in utero with a life of its own, had no special claims to humanity, and no rights they were morally bound to respect. The Romans carried this view to so outrageous an extreme as to hold that the fœtus was a mere excrescence of the mother, a simple appendage from which she could free herself as innocently as she might rid herself of a troublesome disorder. Consequently, abortion became so common in Rome that the greedy quacks who flocked to Rome from all quarters could barely supply the demand for their services and nostrums.

With this condition of things, continue the reporters, Christianity at last waged war. The battle waxed fierce, but the already assured triumph of the purifying faith was postponed by a compromise, how originated or by whom does not appear, no less disastrous than the pagan thing it supplanted. By this compromise it was agreed to consider the fœtus as endued with life only from the date of the maternal sensation called quickening. Abortions forced after "*quickening*" were branded as serious crimes, but all so caused before this period were suffered to pass unnoticed. Henceforth "*quick*" became a word of evil omen. This doctrine,

only a degree less heartless than its pagan predecessor, took a firm hold on society. How effectually it influences the opinion and practice of our time, how completely it has permeated all, but more particularly the higher ranks of contemporary society, need not be insisted on.

The reporters urge that on this subject of quickening there is but one opinion amongst those who are competent to judge upon it, viz. that "*the fœtus is alive from conception, and all intentional killing of it is murder.*" This view fully accepted the legal method of dealing with the crime of abortion would, they maintain, be greatly simplified; the word "quick," which in a legal point of view is the term round which most interest centres, would be abolished, and the simple attempt to procure abortion at any stage of uterine life would stand out clearly as a crime shielded by no technicalities of law.

As the proper name for the intentional destruction of the fœtus is unquestionably "murder," say the committee, it is hoped the time will soon come for its punishment as such. It has been truly and forcibly said, that an induced abortion if undertaken before the viability of the *fœtus*, necessarily contemplates and intends its death, and it may be added the same is nearly certain to result at a subsequent period from injuries inflicted on itself or its mother during the operation. The subsequent death of the mother when, as too often happens, she succumbs to the operation or its consequences, does not change the character of the crime, but rather adds to it the enormity of a double murder. But it is of importance, as fixing the true character of the deed in at least its moral aspect, to bear in mind that an abortionist does not intend the death of the mother. Through all his wickedness and ignorant blundering it is at once his interest and aim to save her life. But he must always and necessarily intend the destruction of the fœtus, if he attempts or induces an abortion before the period of its viability. Consequently the crime is, in regard to the fœtus, an act of cool, deliberate, unrelenting murder, or attempt at murder, and the mother is often undoubtedly *particeps criminis*.

It has been suggested by an eminent legal authority, Judge Bedford, that the offence of producing abortion, for the sake of destroying the fœtus criminally, should be made a capital felony. The reporters object to this punishment of the offence, as calculated to prevent conviction in many cases; and they do not, therefore, advocate any change in the denomination of the crime. The reporters suggest the following amendment of the law as conveying the best scale of punishment:

"Any person who shall administer to any woman with child, or prescribe for any such woman, or advise or procure her to take any medicine, drug, substance, or thing whatever, or shall use or employ, or advise or procure her to submit to the use or employment of any instrument or other means whatever, with intent thereby to produce the miscarriage of any such woman, unless the same shall have been necessary to preserve her life or that of such child; shall, in case the death of such child or woman be thereby produced, be deemed guilty

of a felony, and upon conviction shall be punished by imprisonment in a State prison for a term not less than four years.—*New York Medical Journal*, January, 1872.

SUMMARY.

The Miasmatic Diseases of California. By CAMPBELL SHORB, M.D.—Dr. Shorb says that several years ago miasmatic disease was supposed to reside in fixed geographical limits, growing out of peculiar agencies there existing, occurring at certain seasons of the year, under some atmospheric conditions, generally recognised as necessary preludes to its evolution. It was regarded as a very systematic kind of distemper, always ushered in by the same symptoms, and marked in its progress and termination by characteristics which were found associated with it from the day it was first described in medical literature. In these days, the author contends, miasmatic disease has enlarged its boundaries; it is no longer localised, in fact or in theory.—*The Western Lancet*, February, 1872.

Climate in Relation to Consumption. By MANNING SIMONS, M.D., of Charleston.—The following conclusions respecting the influence of climate on consumption are drawn by Dr. Simons:—

1. That phthisis occurs in every zone, and that its origin is not rendered impossible by any conditions of climate of which we have any knowledge.
2. That the disease, contrary to a very generally received opinion, is not more frequent in cold regions, and that a great degree of cold does not seem to favour its production.
3. The same remark may be made in relation to heat.
4. Of all the influences favorable to the development and progress of consumption, we must recognise moisture, especially combined with heat.
5. Great variability in the qualities of heat, and cold, and moisture are, however, to be recognised as most injurious.
6. The most inimical influence to the origin and development of the disease is that exercised by the climatic conditions and habits belonging to extreme altitudes.
7. Consumption is, as a general rule, more frequently met with on sea-shores, and diminishes, to a certain extent, in proportion to the distance from these localities.—*American Journal of Medical Sciences*, January, 1872.

Five cases of Poisoning occurring in Private Practice. By JOHN DOUGALL, M.D.—These five cases are all of singular interest. In the first case a girl two and a half years old swallowed about a table-spoonful of Adam's patent composition for cleaning furniture, a neutral saponaceous creamy substance, smelling strongly of ammonia and turpentine. After serious symptoms of depression there was recovery. In the second case a boy of two years old took into his mouth a drachm of fuming nitric acid. He spat out at once, and recovered from the local symptoms that were set up. In the third case a boy aged five years recovered from a drachm

dose of oxalic acid. Lime water, magnesia, and warm weak toddy were administered, and recovery was complete in twenty-four hours. In the fourth case, one of poisoning by opium, tartar emetic injected into a vein failed to produce vomiting. Death occurred. The last was a unique case, "resembling hæmorrhagic variola," induced by eating decayed plums. The patient recovered after a long illness.—*Glasgow Medical Journal*, February, 1872.

Toxicological Effects of Chromic Acid. By JOHN DOUGALL, M.D.—The results of the author's experiments show that chromic acid, in concentrated solutions, is a pure and powerful corrosive of animal textures, effecting speedy and complete local disorganization. So actively does it destroy the vascular tunics, gelatinising their fluid contents, that absorption is rendered impossible—these by the merest contact with the acid being converted into consolidated emboli, which choke the capillary passages and preclude further ingress. This view is confirmed by, or may be inferred from, the fact of the poison not being found in the blood or urine. If equal portions of muscular tissue and chromic acid be left in contact for about one hour, the whole is converted into a mass like burnt sugar, which is freely soluble in water, rendering the water yellowish-brown.—*Separate Pamphlet*, Glasgow, 1872.

Physiological Action of Codeia. By REGINALD STOCKER, M.B.—From a series of thirteen experiments with codeia and its polymerides, viz. codeia, dicodeia, tricodiea, tetracodeia, Dr. Stocker concludes that codeia produces cerebral congestion and increased reflex excitability, without vomiting; whilst dicodeia and tetracodeia produce profuse salivation and some depression, with vomiting in several instances, no evidence of cerebral congestion and but little of increased reflex excitability being noticeable.—*From Dr. Wright's Contributions on the History of the Opium Alkaloids. Proceedings of the Royal Society*, April, 1872.

Experiments with Cobra Poison. By JOHN SHORTT, M.D.—Dr. Shortt gives many details of the effects of reputed antidotes against cobra poison, all of which under careful experiments have failed in curative action. He illustrates very forcibly, by experiment, that the poison, when it is administered by the mouth, is not poisonous, and he shows that a female animal yielding milk, if poisoned by the bite of the cobra, does not give a poisonous secretion of milk, even though the young animals continue to imbibe the milk after the death of the mother from the poison. Dr. Shortt uses in his experiments the dried poison, and finds it keeps active for two years at least.—*Madras Monthly Journal of Medical Science*, March, 1872.

[Our experience on the last point is not the same. Some cobra poison in the dried state, brought to us from India by our friend Dr. Gopaul Chunder Sen, was found inactive. It had been kept one year and eight months.—B. W. R.]

CONTRIBUTIONS TO MEDICAL LITERARY HISTORY.

Adversaria Medico-Philologica.

BY W. A. GREENHILL, M.D. OXON.

Part XII.

(*Continued from* vol. xlviii, *p.* 557.)

διεξοδεύω, *to have an outlet or passage.*[1]

διεξοδικός, *belonging or relating to an outlet or passage.* Aristotle uses τὸ διεξοδικόν as a substantive to signify, not only the anus,[2] but the neighbouring parts generally, *including* the nates and the socket of the hip-joint.

διέξοδος, *an outlet or passage* in general, applied to the fæces,[3] to a tendon,[4] and also (theoretically) to air and phlegm.[5]

δῖδρος is explained by Galen in his Glossary of Hippocratic words[6] to mean *moist, transuding* (διϊδροῦσα), but it is not found in any part of the Hippocratic Collection.[7]

δῑδρόω (in the passive voice) is applied by Galen[8] to blood oozing through the pores in the coats of an artery or vein by *transudation;* it probably never means *to perspire,* as stated by Liddell and Scott.

δικέφαλος, *two-headed,* applied to an animal (a monster).[9]—Μῦς ὁ λεγόμενος δικέφαλος, the *biceps flexor cubiti* muscle.[10] The name was probably unknown to Galen, who does not use it where he might have been expected to do so.[11]

δικόνδυλος is applied to the fingers by Aristotle[12] (as μονοκόνδυλος

[1] Hippocratic Collection, 'Epid.,' ii, 3, § 8, tome v, p. 110, l. ult., ed. Littré; perhaps also in the parallel passage, 'Epid.,' vi, 2, § 7, tome v, p. 282, l. 3, where it is adopted by Ermerins ('Hippocr. Opera,' tom. i, p. 267, l. 6). To the authorities in favour of this reading mentioned by Littré it may be added that the passage is thus quoted by Galen ('Comment. in Hippocr. *Epid. II,*' iii, 17, tom. xvii A, p. 422, l. penult.

[2] 'Hist. Anim.,' i, 13, p. 16, l. 2, ed. Bekker.

[3] Hippocr., 'De Fract.,' § 7, tome iii, p. 440, l. penult., ed. Littré; 'Prognost.,' § 11, tome ii, p. 134, l. antep.

[4] Galen, 'De Anat. Admin.,' ii, 8, tom. ii, p, 324, l. 1.

[5] Hippocr., 'Aphor.,' vii, 51, 54, tome iv, pp. 592, 594.

[6] Vol. xix, p. 93.

[7] See 'De Intern. Affect.,' §§ 11, 26, tome vii, p. 192, note 7; p. 234, note 2, ed. Littré; vol. ii, pp. 328, 351, ed. Ermerins.

[8] 'De Meth. Med.,' v, 2, tom. x, p. 311, l. 10.

[9] Aristotle, 'Hist. Anim.,' v, 4, p. 115, l. 23, ed. Bekker.

[10] Meletius, 'De Hom. Fabr.,' c. 27, p. 119, l. 1, ed. Cramer.

[11] 'De Usu Part.,' ii, 16, tom. iii, p. 152, &c.; 'De Musc. Dissect.,' c. 23, tom. xviii B, p. 975, &c.

[12] 'Hist Anim.,' i, 15, p. 17, l. 10, ed. Bekker.

is to the thumb), and signifies *having two knuckles*, κόνδυλος being used somewhat loosely, and not in its strict anatomical sense.

δίκροος, or δίκρους, is explained by Galen[1] to mean δίκρανος, *two-headed*, or δισχιδής, *cloven*. It is applied by him to a muscle,[2] or a tendon.[3] On the connection between δίκραιος, δίκρανος, δίκραιρος, and δίκρους, see note in Franz's edition of Erotianus, 'Lex. Hippocr.,' p. 120.

δίκροτος, *reverberating*, generally used in the ancient medical writers in reference to the pulse, and probably first thus applied by Archigenes.[4] The word *dicrotic* is still found in medical dictionaries, though no longer in common use, and signifies the condition of pulse in which a smaller pulsation suddenly succeeds a larger one, having been compared to a hammer, which, when forcibly struck against the anvil, rebounds, and strikes it again.[5]

δικτυοειδής, *net-like*; δικτυοειδὲς πλέγμα, the name given (apparently) by Herophilus[6] to what is commonly called the *plexus choroides*, situated in the lateral ventricles of the brain;[7] this latter name was also in use in ancient times.[8]

δῖνος, *dizziness*, is found in the Hippocratic Collection,[9] and explained by Galen[10] to apply to a person whose head appears to be running round; when also the eyes were dim, the word σκοτόδινος was used.

διογκοῦσθαι, *to swell*, as barley by boiling;[11] to *be distended*, as the stomach by food.[12]

διόγκωσις, *swelling*, *tumefaction*, a term used (perhaps only, or especially) by the Methodici,[13] stated to be (in the ancient phraseology) a fusion and distension (χύσις καὶ διάτασις) of bodies, and to be the opposite to σύμπτωσις, *depression* (*affaissement*, Daremberg).

διορρόω, *to turn into serum*;[14] in the passive., διορροῦσθαι, *to become*

[1] 'Gloss. Hippocr.,' tom. xix, p. 93.

[2] 'De Anat. Admin.,' iii, 4, tom. ii, p. 369, l. 15.

[3] Ibid., i, 4, 6, 7, p. 240, l. 2.; p. 256, l. 10; p. 259, l. 3.

[4] Galen, 'De Differ. Puls.,' i, 16, tom. viii, p. 537, l. 6.

[5] Adams, Comment. on Paulus Ægineta, ii, 12, vol. i, p. 221, where there is a full account of the opinions of the ancients on the subject of the pulse.

[6] Galen, 'De Usu Puls.,' c. 2, tom. v, p. 155, l. 6. See Marx, 'De Heroph. Vita,' p. 29.

[7] 'De Usu Part.,' ix, 4; xvi, 10, tom. iii, p. 696, l. antep.; tom. iv, p. 323, l. 9; 'De Hippocr. et Plat. Decr.,' vii, 3, tom. v, p. 607, l. 5.

[8] Theophilus, 'De Corp. Hum. Fabr.,' iv, 6, 7, p. 138, ll. 9, 11; p. 139, l. 11. See Bernard's note on Palladius, 'De Febr.,' p. 33.

[9] 'De Victu Acut.,' § 8, tome ii, p. 426, l. 8; 'De Cap. Valn.,' § 11, tome iii, p. 220, l. penult., ed. Littré.

[10] 'Comment. in Hippocr. *De Victu Acut.*,' iv, 35, tom. xv, p. 804, l. 4. See also Erotianus, 'Lex. Hippocr.,' p. 63, ed. Klein.

[11] Hippocr., 'De Victu Acut.,' § 4, tome ii, p. 246, l. 3, ed. Littré.

[12] Id., ibid., § 9, p. 284, l. penult.

[13] Galen, 'De Opt. Sectâ,' c. 28, tom. i, p. 185, ll. 5, 11.

[14] Hippocrates, 'De Morb.,' i, 30. This is the reading adopted by Littré (tome vi, p. 200, l. 14) and Ermerins (tom. ii, p. 177, l. 19); the older editions have διούρησε (tom. ii, p. 209, l. 10, ed. Kühn).

serum, applied to the blood,[1] or *to become whey*, applied to milk,[2] answering to *serescere* in Pliny.[3]

διόρρωσις, *the turning into serum*.[4]

διόσκουροι (Castor and Pollux), probably a *popular* name applied to παρωτίδες,[5] not in the sense of the *parotid glands*, but an *inflammation* or *abscess* of those glands. How old the name is, cannot be decided; but it is unsafe to say (with Kraus, 'Krit. Etym. Medic. Lex.') that it was in use before the time of Galen, as the work in which the word occurs is not one of Galen's genuine writings.

Διοσπολίτης,[6] or Διοσπολιτικός,[7] (sometimes written incorrectly διασπολητικός or διοσπολητικός,)[8] belonging to one of the towns called *Diospolis*, the name applied to a carminative electuary, which was sometimes called in Latin *diacyminum*,[9] in Arabic كموني *kumúní*,[10] from *cummin*, one of its ingredients.

διουρέω, *to pass urine*,[11] and in the passive *to be passed in urine*;[12] also (in the passive) *to promote the secretion of urine*.[13] The word was in the older editions of Hippocrates also applied to the *secretion of semen*,[14] but in these passages both Littré[15] and Ermerins[16] read διορρόω.

διούρησις was formerly found in the Hippocratic treatise 'De Morbis,'[17] but Littré[18] and Ermerins[19] read διόρρωσις, and it is doubtful whether the word is found elsewhere in the old Greek authors, although the word "diuresis" is used by more modern writers in the sense of *an abundant flow of urine*.

διουρητικός, *diuretic*, a word found in the Hippocratic Collection,[20]

[1] Aristotle, 'Hist. Anim.,' iii, 19, p. 76, ll. 5, 29, ed. Bekker.

[2] Id., ibid., c. 20, p. 77, l. 28.

[3] 'Hist. Nat.,' xi, 96, tom. ii, p. 247, l. 26, ed. Tauchn.

[4] Galen, 'Lex. Voc. Hippocr.,' tom. xix, p. 93, l. 15; Hippocrates, 'De Morb.,' i, 30. This is the reading adopted by Littré (tome vi., p. 200, l. 17) and Ermerins (tom. ii, p. 177, l. 24); the older editions have διουρήσιος (tom. ii, p. 209, l. 4, ed. Kühn).

[5] Pseudo-Galen, 'Defin. Med.,' c. 372, tom. xix, p. 440; Eustathius, 'Comment. in Hom. *Il.*,' γ', p. 410, ed. Rom. (quoted by Kühn, 'Opusc. Acad. Med. et Philol.,' vol. ii, p. 336).

[6] Paulus Ægin., vii, 11, p. 113, l. antep., ed. Ald.; Theophanes Nonnus, c. 293; Pseudo-Synesius, 'De Febr.,' p. 268, l. pen.; p. 284, l. 6.

[7] Galen, 'De Sanit. Tu.,' iv, 5; vi, 10, tom. vi, p. 265, l. 11; p. 430, l. 8; Alexander Trall., viii, 13, p. 502, l. 3, ed. Basil.

[8] Leo, 'Consp. Medic.,' in Ermerins, 'Anecd. Med. Gr.,' pp. 97, 310.

[9] Constantinus Afer, in Pseudo-Synesius, 'De Febr.,' p. 39, l. 12; p. 43, l. 1, compared with the two passages above referred to.

[10] Avicenna, vol. ii, p. 200, l. 7, ed. Arab.; lib. v, 1, § 3, vol. ii, p. 286, col. 2, l. 61, ed. Venet. 1608.

[11] Hippocrates, 'De Aëre, Aquis, et Locis,' c. 7, tome ii, p. 38, l. 12, ed. Littré.

[12] Id., ibid., p. 30, l. 4; 'Prorrhet,' ii, 23, tome ix, p. 52, l. 9.

[13] Id., 'De Affect.,' cc. 48, 58, tome vi, pp. 258, 266.

[14] Id., 'De Steril,' tom. iii, ll. 12, 13, ed. Kühn.

[15] Tome viii p. 412, ll. 5, 6.

[16] Tom. ii, p. 657, ll. 20, 21.

[17] Tom. ii, p. 209, l. 14, ed. Kühn.

[18] Tome vi, p. 200, l. 17.

[19] Tom. ii, p. 177, l. 24.

[20] Hippocrates, 'De Rat. Vict. in Morb. Acut.,' c. 14, tome ii, p. 334, l. 1; Dioscorides, 'Mat. Med.,' iii, 69, tom. i, p. 413, l. 8, ed. Sprengel; Aretæus, 'Cur. Acut.,' i, 1, p. 190, l. 4, ed Kühn.

and of which the Latin form *diureticus* was probably introduced into general use in the first or second century after Christ.[1]

διπλασιασμός, doubling or crossing of the arms.[2] Dietz says 'Diplasiasmus fit manibus *in dorso* decussatis,'[3] but it is not certain that the word might not also be applied to crossing the arms *in front*.[4] When *διπλασιασμός* is said in modern works to be applied to the recurrence of the exacerbation of a disease, it is probable that the word is confounded with *ἀναδίπλωσις* or *ἐπαναδίπλωσις*, which words also are rendered *duplicatio* or *reduplicatio* in Latin.

διπλόη, a fold, or *doubling*, of a garment, and (in anatomy) of a membrane, applied to the brain,[5] or the intestines,[6] or the uterus.[7] In an analogous sense it may have signified the double plates of the bones of the skull, but especially the cancellated substance between them. In this last sense the word is found in the Hippocratic Collection,[8] and the name has continued in use ever since.[9]

διπλόος, διπλοῦς, double, was applied to certain forms of intermittent fever, when two paroxysms occurred within a given time, instead of one, as *διπλοῦς ἀμφημερινὸς, τριταῖος, τεταρταῖος*, in the same sense as the names *double quotidian, tertian, quartan*, are still used in the present day.[10]

διπλόω, in the passive *διπλοῦμαι, to become double*, *διπλοῦς*, applied to intermittent fevers.[11]

διπύρηνος, having two nobs: διπύρηνον, used as a substantive, *a probe with two nobs:*[12] written (in the plural) in Arabic دِيبُورِينَا *dibúriná;*[13] but the word does not seem to have been adopted by the Arabians, and is not used by Albucasis in his translation (?) of Paulus Ægineta in two places where we should expect to find it.[14]

[1] Cælius Aurelianus uses the expression, "urinalia medicamina, quæ *diuretica* vocant." 'Morb. Chron.,' i, 4, p. 314, l. 15; p. 318, l. 4.

[2] Galen, 'De Dissect. Muscul.,' c. 21 (19), tom. xviii, B, p. 974, l. 8.

[3] Note in his edition of Galen, 'De Dissect. Muscul.,' p. 45.

[4] Dr. Daremberg, in his translation of Oribasius (who has copied this passage of Galen), renders the word "croisement des bras [sur le dos]," implying by the brackets that the words "*sur le dos*," do not form a necessary part of the explanation of the word *διπλασιασμός*. ('Collect. Medic.,' xxv, 41, § 8, tome iii, p. 454.)

[5] Galen, 'De Anat. Admin.,' ix, 1, tom. ii, p. 710, l. 8, copied by Oribasius, 'Coll. Medic.,' xxiv, 1, tome iii, p. 274, l, 3, ed. Daremb.

[6] Aretæus, 'Caus. Chron.,' ii, 9, p. 157, l. 9, ed. Kühn.

[7] Id., ibid., ii, 11, p. 167, l. 7.

[8] 'De Cap. Vuln.,' §§ 1, 17, tome iii, p. 186, l. ult., p. 250, l. 1, ed. Littré.

[9] Aretæus, 'Cur. Chron.,' i, 2, 4, p. 298, l. antep., p. 310, l. 6, ed. Kühn; Rufus Ephes., 'Part. Corp. Hum.,' p. 34, l. penult.; Galen, 'De Meth. Med.,' vi, 6, tom. x, p. 445, ll. 5, 8, 15.

[10] Galen, 'De Typis,' c. 5, tom. vii, p. 472, l. 5, &c.; 'De Cris.,' ii, 9, tom. ix, p. 677, l. 12, &c.; Leo, 'Consp. Medic.,' i, 6, in Ermerins, 'Anecd. Med. Gr.,' p. 95.

[11] Galen, 'De Typis,' c. 5, tom. vii, p. 472, l. 3.

[12] Cælius Aurelianus, 'Morb. Acut.,' iii, 3, p. 187, ed. Amman; Galen, 'De Anat. Admin.,' vi, 10, 13, viii, 4, tom. ii, p. 574, l. 9; p. 581, l. 9; p. 667, l. 18. At p. 728, l. 83, for *διπυρήνην* we should probably read *διπύρηνον*.

[13] Honain's MS. translation of Galen, 'De Anat. Admin.'

[14] Paulus Ægin., vi, 77, 78, p. 312, l. antep., p. 322, l. 14, ed. Briau; Albucasis, 'De Chirurg.,' ii, 86, 80, pp. 398, 358.

διασκοειδής, like a quoit, applied to the seed of the *Thlaspi bursa*, or shepherd's purse:[1] *ὑγρὸν διασκοειδές* was one of the names given to the crystalline lens.[2]

διστιχία, a double line, as *διστιχία τριχῶν, a double row of eyelashes*;[3] sometimes the affection of the tarsus in which this occurs is called *διστιχία* (without the addition of *τριχῶν*),[4] sometimes *διστιχίασις*.[5]

δισχιδής, split in two, cloven-footed, applied to animals like the sheep or goat, as distinguished from *ἀσχιδής* and *πολυσχιδής*;[6] applied also to a tendon,[7] an artery,[8] or a vein[9].

διτριχιάω, to have a double row of hair, applied to the tarsus when affected with *διστιχία*.[10]

διχαστήρ, a divider, probably found only in the plural *διχαστῆρες*, a name given by some later Greek writers to the *incisor teeth*,[11] because they divide (*διχάζουσι*) the food.

δίχηλος, having cloven hoofs, opposed to *ἄχηλος, having solid hoofs*.[12]

διψακός, one of the names given to *diabetes*, on account of the *thirst* attending it.[13]

διψώδης, thirsty,[14] also *exciting thirst*,[15] as applied to wine, &c.

διωσμός, pushing through, applied to extraction of a foreign body from a wound by pushing it forward, rather than by drawing it back (*ἐφελκυσμός*).[16] In Aretæus[17] *διωσμος χειρῶν* is rendered by Crassus "manuum agilitas;" by Petit, "manuum dispulsio;" by Freind, "manuum circumquaque protrusio;" and by Adams, "strong exercise of the hands," which perhaps expresses the general sense with tolerable correctness.

διωστήρ, the name of the instrument used for *διωσμός*, the propulsion of a foreign body through a wound.[18] One end, called the *male* part, *ἄρρην*, was solid; the other end, called the *female* part, *θήλεια*,

[1] Dioscorides, 'Mat. Med.,' ii, 185 (186), tom. i, p. 296, l. 6, ed. Sprengel.
[2] Rufus Ephesius, 'De Appell. Part.,' p. 55, l. 20, ed. Clinch.; Aëtius, vii, 1, p. 123, ed. Ald.
[3] Pseudo-Galen, 'Introd.,' c. 16, tom. xiv, p. 771, l. antep.; Leo, 'Consp. Med.,' iii. 15, in Ermerins, 'Anecd. Med. Gr.,' p. 135.
[4] Pseudo-Galen, 'Defin. Med.,' cc. 359, 360, tom. xix, p. 438, ll. 2, 3; Paulus Ægin., vi, 11, p. 112, l. 10, ed. Briau.
[5] Aëtius, ii, 3, 66, p. 328, ed. H. Steph.; Paulus Ægin., vi, 8, p. 100, l. 16.
[6] Aristotle, 'Hist. Anim.,' ii, 1, p. 29, l. 16, ed. Bekker.
[7] Galen, 'De Anat. Admin.,' ii, 8, tom. ii, p. 323, l. 9.
[8] Ibid., vi, 6, p. 567, l. 5.
[9] Ibid., iii, 5, 12, p. 378, l. 9; p. 382, l. 14; p. 407, l. 11; p. 408, l. 18.
[10] Pseudo-Galen, 'Introd.,' cap. 16, tom. xiv, p. 771, l. 11.
[11] Julius Pollux, 'Onomast.,' ii, 4, § 91; Pseudo-Hippocr., 'Epist. de Hom. Fabr.,' in Ermerins, 'Anecd. Med. Gr.,' p. 289, l. 1.
[12] Galen, 'De Anat. Admin.,' iv, 3, tom. ii, p. 430, l. ult.
[13] Galen, 'De Locis Affect.,' vi, 3, tom. viii, p. 394, l. 13; Alexander Trall., ix, 8, p. 552, l. 29, ed. Basil.; Paulus Ægin., iii, 45, p. 47, l. 46, ed. Ald.
[14] Aretæus, 'Caus. Acut.,' ii, 4, 12, p. 41, l. 14; p. 64, l. 14, ed. Kühn.
[15] Hippocrates, 'De Vict. Acut.,' § 14, tome ii, p. 332, l. 9; p. 334, ll. 3, 4, ed. Littré.
[16] Paulus Ægineta, vi, 88, p. 350, ll. 13, 18, ed. Briau.
[17] 'Cur. Chron.,' i, 3, p. 306, l. 14, ed. Kühn.
[18] Paulus Ægineta, vi, 88, p. 352, ll. 15, 20.

was hollow. The Arabic name used in the translation of Paulus Æginetа by Albucasis is مدفع *madfa'*, and there are drawings of the instrument in Channing's edition,[1] and in Leclerc's French translation (figs. 135, 136).

δοθιήν, a word not used in a very strictly defined sense, translated by Littré *bouton*[2] (or *pimple*), by Adams *furunculus*[3] (or *boil*); probably both meanings were included. Two kinds of δοθιήν were described,[4] of which the deeper-seated variety differed from φῦμα (*tubercle ?*) only in hardness.[5] Galen mentions this as one of the names of diseases which are derived neither from the part affected, nor from the cause of the malady.[6] This is one of the words examined by Kühn in his 'Censura Lexicorum Medicorum recentiorum.'[7] The principal passages where it is mentioned by Galen may be found in Kühn's Index under 'Furunculus.'

δυθιηνικόν, the name of a medicine mentioned by Paulus Æginetа[8] as useful for boils or pimples, the composition of which is not given.

δοθίων, probably a later form of the preceding word, which is *not* recognised by Liddell and Scott in their Lexicon, but which is found in Leo,[9] and perhaps also in Palladius.[10]

δορκαδίζω, *to bound like an antelope.* The word δορκαδίζων was applied by Herophilus[11] to the pulse when the dilatation of the artery is interrupted, and then hurriedly completed; and the idea was derived from the antelope, which in jumping was said to take first a short spring, then to seem to make a sudden stop, and then to take a much larger and swifter bound. The similitude was adopted by the Arabic writers, who used the word غزالي *gazáli*,[12] and the words *caprizans*, *dorcadissans*, *gazellans*, are found in the Latin translators. A full account of the complicated doctrine of the pulse and its different varieties may be found in Paulus Æginetа,[13] and Adams's Commentary. The account given by Galen (which is copied more or less exactly by the other ancient writers) may be seen by consulting Kühn's Index, art. 'Caprizans pulsus.'

[1] 'Chirurg.,' vii, 94, p. 458.

[2] 'Hippocrates,' 'Epid. VI,' iii, 23; 'De Humor.,' § 20, tome v, p. 304, l. 3; p. 500, l. 9; 'De Affect.,' § 35, tome vi, p. 246, l. 6.

[3] Paulus Æginetа, iv, 23, vol. 2, p. 73.

[4] Galen, 'De Compos. Medic. sec. Loc.,' v, 3, tom. xii, pp. 824, 825.

[5] Id., 'De Tumor. præter Nat.,' c. 15, tom. vii, p. 729, l. 3.

[6] 'De Meth. Med.,' ii, 2, tom. x, p. 83, l. 3.

[7] Reprinted in his 'Opusc. Acad. Med. et Philol.,' vol. ii, p. 373.

[8] iv, 23, p. 65 B, l. 18, ed. Ald.

[9] 'Consp. Medic.,' vii, 8, in Ermerins, 'Anecd. Med. Gr.,' p. 209.

[10] 'Comment. in Hippocr. *Epid. VI*,' in Dietz, 'Schol. in Hipp. et Gal.,' tom. ii, p. 101, l. 7, where both the Greek MSS. read δοθίονες, though the editor has introduced δοθιῶνες into the text.

[11] Galen, 'De Differ. Puls.,' i, 28, tom. viii, p. 556, l. 1.

[12] Avicenna, i, 2, 3, vol. i, p. 63, l. 48, ed. Arab.; vol. i, p. 142, l. 57, ed. Venet. 1608.

[13] ii, 12, vol. i, p. 202, &c.

δόσις, a *gift*, a *portion*, a *dose* of medicine;[1] also the *giving* or *exhibition* of a medicine.[2]

δρακόντιον, the species of worm called 'Filaria Medinensis,' or 'Guinea-worm,' of which the earliest notice is probably that by Agatharchides[3] in the second century B.C. It was mentioned by Soranus and Leonidas,[4] and also by Galen, who says that he had never himself seen one.[5] It is described by the Arabic writers under the name عرق المديني *'irku-l-mediní*, 'vena Medinensis,'[6] so called from the city of Medina, because it was more commonly found in those parts of Arabia. There was in early times (as might be expected) considerable difference of opinion as to the nature of the substance found in the swelling, some persons considering it to be a sort of worm, and others that it was corrupted animal matter. For further information (especially the antiquarian part of the subject) see the treatises by G. H. Welschius,[7] and Justus Weihe,[8] and Adams's Commentary on Paulus Ægineta, vol. ii, p. 151.

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[1] Aretæus, 'Cur. Acut.,' i, 10, p. 240, l. penult.; 'Cur. Chron.,' i, 8, p. 304, l. 8, ed. Kühn.

[2] Theophanes Nonnus, 'De Cur. Morb.;' c. 33, vol. i, p. 134, l. 8, p. 136, l. 3, ed. Bernard.

[3] Plutarch, 'Sympos.,' viii, 9, § 3, vol. iv, p. 426, l. 3, ed. Tauchn.

[4] Paulus Ægineta, iv, 59, p. 73, l. 42, ed. Ald.; Aëtius, iv, 2, 85, p. 736, ed. H. Steph.

[5] 'De Locis Affect.,' vi, 3, tom. viii, p. 393, l. 1. See also 'Introd.,' c. 19, tom. xiv, p. 790, l. penult.; 'Defin. Med.,' c. 437, tom. xix, p. 449, l. 8.

[6] Avicenna, iv, 3, 2, vol. ii, p. 76, l. 29, ed. Arab.; vol. ii, p. 132, ed. Venet., 1608.

[7] 'De Vena Medinensi,' &c., August. Vindel., 1674, 4to.

[8] 'De Filar. Medin. Comment.,' Berol., 1832, 8vo.

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Revista Medico-Quirurgica. Ann I. February, 1872.

CORRIGENDA.

In No. XCVIII, April, 1872, p. 500, *for* "John A. Ogle," *read* "John W. Ogle."

In No. XCVII, January, 1872, correct as under, in Dr. Karl B. Hofmann's original essay on the "Recent Progress of Uroscopy."

Page 193, line 25, *for* "$2C_2H_2O$," *read* "$2C_2H_4O$."
" " " 28, *for* "Carbonate," *read* "Carbaminate."
" " " 33, *for* "150°," *read* "140°."
" 194, " 4, *for* "Béchamps," *read* "Béchamp," and so throughout the page.
" 196, " 6 from bottom, *for* "and he excreted," *read* "and three hours later he excreted."
" 197, " 10, *for* "3·06," *read* "1·06.
" 198, note 1, *read* "Uber Einwirkung Kalter Bäder auf CO_2- und Harnstoff-Ausscheidung beim typhus." 'Deutsch. Archiv für Klinische Med.,' iv, 385, a. f.
" 199, line 11, *for* "seven," *read* "severe."
" 200, bottom. At end of last note *add* "Heft iii."
" 201, line 7, *for* "1·99," *read* "1 : 99."
" " " 4 from bottom, *for* "L. Riess and A. Valenta," *read* "L. Riess[1] and A. Valenta."[2]
" " last line, *for* "Riess,[2]" *read* "Riess."
" " at end of note 1, *for* "183 a. f.," *read* "XV, 1—118."
" " at end of note 2, *add* "183, a. f."
" 203, line 8, *for* "(on an average 1 grain to 3 grains, and 21·8 grains)," *read* "(on an average in the first case 19·3 grammes, and in the second 21·8 grammes)."
" " " 17, *for* "39·4°," *read* "39·70."
" " bottom line, *for* "1·10—1·14," *read* "1010—1014."
" 205, line 3, *for* "(1—2 and 1—3)," *read* "(1 : 2 and 1 : 3)."
" " " 5, *for* "28·30 grains," *read* "28·9 grammes," and *for* "0·71 grains," *read* "0·91 grammes."
" " " 8 *for* "uric acid, urea = 1·31—70," *read* "uric acid ; urea = 1 : 31·7."
" " " 9, *for* "1·60—80)," *read* "1 : 60—80 ; the."
" " " 10, *for* "0·50 grains," *read* "0·50 grammes)."
" 206, " 10, *for* "1·30," *read* "1 : 30."
" " " 21, *after* "were found," *insert* "by other experimenters."
" " lines 29 and 30, *for* "Mehn," *read* "Mehu."
" " line 3 from bottom, *for* "E. Hefsen," *read* "Edlefsen."
" " note 2, *substitute* "'Deutsches Archiv für Klinische Medizin,' vii, 67, a. f."
" 207, line 18, "20—26 gr.," *read* "20—60 grammes."
" " note 1, *substitute* "'Virchow's Archiv,' lix, 437."
" 208, " " *add* to the note "N. F. I., 187."

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1872.

Analytical and Critical Reviews.

I.—Painters and the Accidents of Sight.[1]

THOUGH it had in various ways come under our notice that a lecture delivered in March last at the Royal Institution, by Mr. R. Liebreich, on "Turner and Mulready" in which he had resolved their later styles of painting into symptoms of ocular diseases of which they themselves were unconscious, had produced a sensation in art-circles; it was not until several weeks after its publication in the April number of 'Macmillan's Magazine,' that the lecture itself fell into our hands. That æsthetical witticisms should have rained upon the lecture is no more than might have been foretold. We do not perceive that the physiological and pathological principles on which he bases his propositions have excited any discussion. Rather than that their accuracy shall be tacitly endorsed, we will take it upon ourselves to review the lecture. Besides, supposing it to have been already reviewed in this sense, we are persuaded that our comments will not, in the main, have been anticipated by previous reviewers. It is our intention to accept his challenge of considering his

1. [1] *Turner and Mulready. On the Effect of certain Faults of Vision on Painting, with especial Reference to their Works.* By R. LIEBREICH, Ophthalmic Surgeon and Lecturer at St. Thomas's Hospital. (Lecture delivered at the Royal Institution on the 8th March, 1872.)
2. *The Life of J. M. W. Turner, R.A.* By W. THORNBURY.
3. *Ueber die entoptische Wahrnehmung der Netzhaultgefässe, &c.* Von. H. MÜLLER. Verhand d. Phys. Med. Ges. in Würzburg, 1855.
4. *Entoptics.* By JAMES JAGO, M.D. Oxon., A.B. Cantab. (now F.R.S.).
5. *Physiologische Optik.* Von H. HELMOLTZ, Professor of Physiology.
6. *On the Anomalies of Accommodation and Refraction of the Eye.* By L. C. DONDERS, M.D., Prof. of Physiology. N. Syd. Soc.
7. *Bakerian Lecture.* By THOMAS YOUNG, M.D., &c. Phil. Trans., 1801.
8. *On Colour-Blindness.* By W. POLE, F.R.A.S. Phil. Trans., v. 149.
9. *On Colour-Vision. A paper at the Royal Institution.* By Prof. J. CLARK MAXWELL, M.A., LL.D., F.R.S. ('The Engineer,' May 26th, 1871.)

arguments from a "purely scientific point of view," without confining ourselves to this aim; for the perusal of his lecture has generated within us such devious speculations, that we shall not be able to refrain from taking our readers into our confidence with respect to some of them.

First of all, it is but just to the lecturer that it should be unreservedly admitted that he has clearly entered upon the new field of research which he has the merit of indicating with patient honesty. For, though it is an oft told tale that Turner could not see to paint befitting his fame during his last few years, and that faults of vision plagued poor Haydon and other painters, the project of analyzing the pictures of artists upon optical principles in such a way as to write the history of their eyes—to assign in this way at any given date the states of the sundry parts of those complicated organs of theirs—is undoubtedly entirely his own. To an ophthalmologist who is conscious that he has spared no pains to make himself master of whatever preliminary knowledge may be attainable to qualify him for such an investigation, the ardent pursuit of it cannot be otherwise than captivating. As a man of science he is engaged in accumulating facts in verification of a hypothesis which has emanated from himself; as one whose talents are pledged to the noble endeavour to preserve the sight of his patients, or to restore it when damaged or lost, he must be ever acquiring greater nicety in the art of diagnosis, and ever striving to devise more efficient means of treating ocular diseases or more cunning operations for removing them. And if a lover of the art of painting he is cheered on by the hopes that he may even, in the course of his practice, rescue some future Turner or Mulready from unconsciously wasting his matured powers on productions that discredit a glorious reputation already earned. Again, no one has so good opportunities as an oculist in guarding against errors in compiling such records; for, if he has, perchance, doubts as to the visual defects which have left their impress on a landscape or cartoon, he will probably be able among his numerous patients to find some one who sees things in like guise as the artist had seen, and whose eyes he can directly examine and test.

Furthermore, if there be wisdom in the axiom that every man should have a hobby as well as a vocation, surely the author could mount none less likely to render him chargeable with becoming oblivious of his social obligations than that of narrating, by reference to works of art treasured up or accidentally still existing, the diversified manifestations of the anomalies of sight from the time of the pyramids to our own. We can even conjecture that some day an anthropologist may arise who would strive to decide from pictorial productions whether the Egyp-

tians and Peruvians had similar or dissimilar eyes, that is, were or were not of the same race. We have ourselves seen rickety bones and decayed teeth in a mummy, and felt thus fortified against those who preach that the human race is deteriorating in physique, but we never hoped to have proof that Egyptians with bad eyes were held to their tasks by fondness for their art or by inexorable masters. Nor did we dream that anti-Darwinians might draw specious arguments from such a source of inquiry. There is nothing fanciful in these suggestions, for if sound rules can be established for drawing just inferences from the inquiry in question, all this and much more will be attempted by its aid.

But let us not forget that there are other folk beside anatomists concerned in the issue. Mr. Ruskin and the host of art-critics must have received a torpedo-shock when apprized that a great portion of their voluminous eloquence, like the showy double flowers of horticulturists, are incapable of fructification. To spend one's subtlety in struggling to perceive the mental process by which a favourite artist arrives at a style of drawing or colouring which he never would have adopted could he have seen what he was doing, what more humiliating destiny could befal a man of genius? It is to be hoped that the more rational of such infatuates will spare no diligence in preparing themselves for the elimination of the errors alluded to from their writings; that is, by beginning at the beginning; by educating themselves to comprehend the more commonplace elements of criticism preliminarily to studying art and artists' minds ideally.

Our imagination is of too poor an order for us to attempt to fathom the abyss of forebodings and misgivings into which the painters must have been plunged by the unwelcome announcement. Would that some poet could find inspiration enough for the subject! We cannot think both of poets and of controverted blindness without recalling Homer to our recollection. Would not the opening of his 'Odyssey' serve again for the present occasion?

> Ἄνδρα μοι ἔννεπε, Μοῦσα, πολύτροπον, ὃς μάλα πολλὰ,
> Πλάγχθη, ἐπεί

Or was Virgil more happily prophetic of the fate of the versatile Turner, great in marine as in landscape painting?

> Multum ille et terris jactatus et alto.

Perhaps the painters themselves might handle the subject with success. What pathos might be thrown into a cartoon for next year's exhibition at the Royal Academy, wherein should be a beautiful landscape with the unclouded sun in the middle —such as Turner loved to contemplate — done in a manner like his best productions, and in the foreground the artist him-

self, in serene composure, occupied in reducing the same to canvas, "*naïvely*" representing the glorious orb with "a vertical yellow streak" of light emanating from it, "dividing his picture into two entirely distinct halves," not doubting that he was faithfully copying nature!

But there it is, no artist could generously devote his abilities to cause his countrymen to cherish an immortal sympathy for their great landscape painter in his days of infirmity without running the risk of jotting down somewhere the shadow of a mote, a vagrant *musca volans* (*exempli gratiâ*) in his own vitreous humour, on or near the beam that indicated the defect in another's eye.

Should we confine our thoughts strictly to such artists as are alive and kicking, we should like to be informed whether the mischievous tribe of caricaturists survives; for if so these heartless wags would not stick at ridiculing the absurdities that flow from each others unsuspected ocular anomalies, and thus gibbet any one of their number who might betray a peculiarity of his own whilst rolicking over a peculiarity in an acquaintance, and so on: the gibbeter being the next to be gibbeted, for

> "These fleas have other fleas to bit'em,
> "And these fleas fleas, *ad infinitum*."

Prima facie there seems no inherent impossibility in the successful study of paintings by Mr. Liebreich's method: but if we accompany him into his details we shall discover that his line of march lies among quicksands. He says:

"Till the year 1830 all is normal. In 1831 a change in the colouring becomes for the first time perceptible, which gives to the works of Turner a peculiar character not found in any other master. Optically this is caused by an increased intensity of the diffused light proceeding from the most illuminated parts of the landscape. This light forms a haze of a bluish colour, which contrasts too much with the surrounding portion in shadow. From the year 1833 this diffusion of light becomes more and more vertical from the year 1839 the regular vertical streaks become apparent to every one. It is the generally received opinion that Turner adopted a peculiar manner, that he exaggerated it more and more, and that his last works are the result of a deranged intellect. I am convinced of the incorrectness, I might almost say, of the injustice of this opinion. According to my opinion, his manner is exclusively the result of a change in his *eyes*, which developed itself during the last twenty years of his life. In consequence of it the aspect of nature gradually changed for him, while he continued in an unconscious, I might almost say in a *naïve* manner, to reproduce what he saw. And he reproduced it so faithfully and accurately that he enables us distinctly to recognise the nature of the disease in his eyes, to follow its development step by step. After he

had reached the age of fifty-five, the crystalline lenses of Turner's eyes became rather dim, and dispersed the light more strongly, and in consequence threw a bluish mist over illuminated objects. This is a pathological increase of an optical effect, the existence of which, even in normal eyes, can be proved by the following experiment. If you look at a picture which hangs between two windows, you will not be able to see it distinctly, as it will be, so to speak, veiled by a greyish haze the light of the windows had been diffused by the refracting media of the eye, and had fallen on the same part of the retina on which the picture was formed."

Now this is the first illustration with which the lecturer supports his pathological conclusions from physiological vision, and we stop to take note of it as likely to afford us some gauge of the extent of his acquaintance with the bye-ways of physiological optics from which his reasoning must proceed. It is singular that this explanation of his of the manner in which the light from the windows prevents our seeing the picture that is hung between them is, demonstrably, not to mince words, a blunder in the said optics. It is in vain that he cites experiments to show that no medium whatever—not even the "most beautiful eye"—is perfectly transparent; not only is this truth trite, but if he had added that the tunics of the eye themselves are permeable by light no one would have gainsayed the affirmation. But in this instance the dispersions of light in the refracting media of the eye, are in no appreciable degree the cause of the dazzling in question, for were there absolutely no dispersion the dazzling would be greater instead of less than it now is.

Our first care when we would wish to view a picture undazzled which is hung against the wall of a room lit by "two windows," must be so to place ourselves that no line can be drawn from either of the windows to any point in the picture that makes with the normal to its surface thereat the same angle that the optic axis makes that is directed to that point, all three lines being supposed to be in one plane. Otherwise, speaking summarily, in the glass that covers a water-colour drawing a plain image of the window, and in the irregular varnish surface of an oil painting, a lustre, being a wavy, disordered image of the window, will fall upon the same portion of the retina whereon the picture is imaged, and quench its image.

This condition being secured, our next care should be that the windows be hid from the eyes, if our back is not towards them, lest their images should be formed (no matter whether definitely or indefinitely) on the retina, though it be only at a distance from its central portion where the picture is imaged. Because when this is not provided against the portions of the retina, illuminated by the images of the windows

and the tunics covering it, as none of these are smooth polished surfaces, become radiant, so that rays of light issue therefrom in every unobstructed direction. Thus, in the case of the picture hung between two windows, the retinal images of these emit so many rays upon the image of the picture in the middle portion of the retina as to partially extinguish it: not to mention that these retinal images are reflected by the curved posterior surface of the crystalline lens back to the seat of the image of the picture, to actually cover it with diffused images of themselves, that is of the windows, &c. It can be shown that it is this retinal radiation that is the fundamental cause of the dazzling, which the lecturer assigns to lenticular dispersion of light.

Let it be night, and let the window-shutters be closed, and let a candle furnish the sole light in the room, and standing some way back from the picture, let us fix the eye (the other being shut) upon a point of it that is equally distant from the two windows, and then, holding the candle, say about a foot from the face, in the line between the eye and the middle of either of the windows, carry its flame in a circle around the optic axis, and keep on doing so in the same circle. In this way the flame projects light upon the retina precisely where the windows had projected their light, the retinal image of the flame passing alternately over the former sites of the retinal images of the windows, as it keeps on describing a circle round the point of direct vision in the *foramen centrale.* What happens? As the candle circulates we see strikingly manifested the shadows of the central vessels of the retina, which ramify along it in *front* of the actual sentient layer of that tissue, and also a partial shadow of the wall of the central hole, circulating over the picture round the point on which the eye is fixed. Again, screen the pupil of the eye from the candle, and look aside at any very faintly lighted surface, and let an assistant concentrate the light of the candle upon the white of the eye (sclerotic coat) by means of a lens of short focus, and let the lens whilst he is doing this be moved to-and-fro over the eyeball. Then the vessels with the rim of the foramen will be seen in movement, as before.

These are the celebrated Purkinje experiments. And what is their history? Shortly after he had published an account of them, believing that the phenomenon was from some unknown nervous cause, Wheatstone suggested they must somehow be real shadows of the retinal vessels. Then H. Müller showed, by studying the movements of the shadows, that they were thrown by rays issuing from the retinal image of the flame, or the tunical when introduced through the sclerotic, or more

properly, the image with which both retina and tunics in each case is imbued. The last writer went so far as even to calculate the distance of a vessel, which he presumed to be on the border of the yellow spot in his eye (most eyes being found to have vessels thereabouts) from the sentient layer of the retina, by observing the angular motion of its shadow when the focus of the lens had travelled a measured distance along the sclerotic.

Lastly, Dr. Jago dispenses with any assumption as to the site of a retinal vessel, giving a method of determining this in the case of any such vessel whose shadow can be seen revolving, as also its distance from the true sentient surface. He does the same for the brim of the central hole. He observes the apparent diameter of the circle in which the candle is moved, and of the circle of the revolving shadow, and, assuming nothing to be known as to the eye but the length of its optic radius, provides us with a simple trigonometrical formula, which at once determines the measurements sought. Moreover, all persons who have experimented by circulating a light in front of the eye, or by concentrating light on the sclerotic, have noticed a peculiar extra brightness moving over the middle of the visual field, which has obtained the name of the "shagreened appearance." It was thought to depend upon some difference between the central and surrounding portions of the retina, except by H. Müller, who guessed it might be due to reflection. Dr. Jago proved that this was a diffused image of the image of the candle on the retina, and ocular coats including it, cast by reflection at the posterior surface of the crystalline lens upon the middle of the retina as a shagreened spot. Of course rays from the retinal images of the flame are also reflected at the anterior surface of the crystalline, and at the cornea, so as to impinge upon the middle of the retina, and re-reflected at the surface of the retina, &c. But the disturbance to clear vision, superadded in these ways, is too subordinate to be worth dwelling upon. It suffices that we have made it palpable that it is the radiations from the retinal images of the windows and the shagreened images of their images that constitute the essential detriment to our seeing the picture hung between the windows. Therefore the lecturer's citation of the fact as a proof of lenticular dispersion of light is of no help to his argument, and he thus remains without any physiological support whatever when he affirms that diseased crystalline lenses can produce the vertical diffusion of light which he notices in Turner's pictures.

But when he tells us that it was because "a clearly defined opacity was formed in the slight and diffused dimness of the crystalline lens" that light was "principally dispersed in a vertical direction," we answer that such an effect from such a

cause is to us inconceivable. To ascribe such an effect to the lens we should be obliged to imagine it not changed in constitution, but in form, so as to present very deep, horizontal concavities near its upper or lower margin on one or both of its faces; to have suffered in fact such an alteration as has never been known to occur in the lens.

We have physiological experience for our guidance, which will make this matter readily comprehensible. The conjunctival surface of the eye is covered with lubricating fluid, and it is at this fluid that rays of light entering the eye undergo their first refraction, a refraction far greater than they undergo even at the surfaces of the crystalline lens, so great a difference is there between the densities of this fluid and that of the atmosphere. Now, this fluid lies along the borders of the eyelids against the eye, in such a manner that its anterior surface there along is actually concave vertically, instead of convex, as it is generally over the cornea. The consequence is, that all the rays of light that enter the eye near the margins of the lids, instead of being made to converge, as those entering the cornea generally, become less convergent, or are rendered more or less divergent, in a direction perpendicular to the edges of the lids; that is, in the upright position of the head, vertically. Ordinarily such diverted rays are intercepted by the iris, but whenever either lid covers a portion of the pupil—whenever the globe of the eye is turned strongly in its socket to look upward or downward, or the lids are partially closed, or the pupil unusually large, a segment of the pupil may be covered—in such case the fluid along the upper lid occasions a long downward beam of light to issue from any luminous disc we are looking at, and that along the under lid a long upward beam. The beams spread a little as they lengthen, and though the light of the disc should be white, ordinarily manifest more or less perfectly the colours of the rainbow, with the most refrangible in the middle of the beam, and the least so at the sides—an effect of ocular refractions. It depends upon the relative directions of their eyelids whether the beams from the two eyes shall be coincident or not.

The account that the lecturer gives of the streaks in Turner's pictures apply equally well to those we have instanced. By the devices above detailed we can draw out a "yellow streak" from the sun down across a landscape: in this way "the horizontal line of demarcation between house and water, or boat and water, entirely disappears, and all becomes a conglomeration of vertical lines." Again, we obtain the vertical diffusion of "bluish" light. An opacity of the lens plainly furnishes none of the conditions for the production of such streaks.

Inasmuch, however, as the size of the pupil is greater as the retina is less stimulated by light, lenticular opacity might lead to an undue expansion of the pupils underneath one or both lids, and thus *indirectly* lead to the visibility of the streaks; and so may other maladies in the interior of the eye. An inflamed iris might adhere, whilst in a state of contraction, to the capsule of the lens, so that a broad pupil would exist ever after. We are not aware that any oculist would have practised such an operation in Turner's day, but if we could imagine Turner, who rarely betrayed his movements to any one, to have slipped across Channel, and to have returned with portions of his irides snipped out under his upper eyelids, he would have had a beam as the accompaniment of vision to the last day of his life, whether von Graefe and his disciples ever contemplated such a contingency or not. In a word, though unable to accept the lecturer's solution of the problem, it is easy enough to imagine Turner to have really copied the streaks he draws or paints, whether through putting himself in a certain posture whilst sketching, or through having some ocular peculiarity. Singularly enough, Mr. Mayall tells us that "Turner stooped very much, and always looked down;"[1] that is, in surveying a landscape he would, *relatively*, have the pupils turned upwards under the upper eyelids, and would thus engender, at the sun and other bright surfaces, long streaks of gauzy light descending over less bright surfaces beneath them in the landscape.

Probably Mr. Liebreich himself does not mean that Turner could have been so deceived as to the direction the streaks of his brush were taking. Unless, then, his streaks proceeded from a growing and vicious habit of using his brush, or that he did not design his pictures for close inspection, we are driven to surmise that he intended to paint what he saw, and succeeded in doing so; as Mr. Liebreich touchingly affirms.

But then the curious question remains—was Turner so unacquainted with subjective phenomena of vision that he believed the streaks to be veritable, if to him inexplicable, aspects of natural scenery, or did he know that they were subjective, and introduced them into his pictures with a purpose? If his mind

[1] The Life of J. M. W. Turner, R.A., by Walter Thornbury, v. ii, pp. 259-63, gives Mr. Mayall's account of Turner's visits to his photographic *atelier* in 1847, '48 and '49; he speaks of "his weak eyes and their being rather bloodshot." This sole description of the diseased appearance of his eyes which we can find would lead us to conjecture that his complaint was mainly watery eyes, that troubled him without preventing his seeing "fleeting shadows," and delicate differences of colour. It was for the purpose of observing such niceties that he so frequented the studio. (p. 273.) In the last year of his life (1851) his health rapidly failing, Mr. Roberts says of him, "As he stood by the table in my painting-room I could not help looking attentively at him, peering in his face, for the small eye was as brilliant as that of a child, unlike the glazed and lack lustre eye of age."

was "deranged," it matters not which view we accept, but if his mind was sound, the question should not be shunned. It is plainly one of much interest. We will return to this topic after following the lecturer through his other topics.

After having arrived at his induction of the characteristics of Turner's vision from those of his pictures, the lecturer goes on to give us an exposition of some ordinary diversities among eyes physiologically transparent; as in the normal, the short and oversighted eye. In this popular summary there is nothing to cavil at, though, when he affirms that a "small handwriting is an indication of shortsightedness," and "that artists who paint small pictures. . . with great minuteness. . . are mostly short-sighted," we think it well to state that probably the size and length of bone with the muscular development of the hand and forearm, and early training have influence upon the degree of nicety in the use of pen, pencil, or brush. At all events we know shortsighted correspondents who write large hands, and normal and even oversighted (using spectacles) ones who write small: and we believe there have been exquisite miniature painters and engravers who were not shortsighted. However, we do not wish to insinuate that his remark has not much justice in it.

Except that a licence is permissible in language intended for an uninitiated audience we should demur to his narrow definition of astigmatism (focuslessness); "sometimes the shape of the eye diverges from the normal spherical form, and this is called astigmatism."

However, we are most interested in any examples he may furnish us with, and we waive all minor objections to reach them. We may clearly put in a shorter way of our own his description of the cornea in his two cases of astigmatism by stating that it is not a portion of a sphere, or a spheriod, but the apex of an ellipsoid, whose longest and shortest diameters are perpendicular to each other.

"I know a landscape painter and a portrait painter who have both the same kind of astigmatism; that is, the refraction of the vertical meridian differs from the refraction of the horizontal one. The consequence is that their sight is normal for vertical lines, but for horizontal lines they are slightly short-sighted."

The pictures of the first show some insignificant blemishes in horizontal shading which need not detain us. The portraits, however, display a serious blemish.

"The portrait painter, in whom a moderate degree of astigmatism manifested itself at first only by the indistinctness of the horizontal lines, has now becomes far-sighted for vertical lines, and therefore sees a distant person elongated in a vertical direction; his picture,

on the contrary, being at a short distance, is seen by him enlarged in a horizontal direction, and is thus painted still more elongated than the subject is seen; so the fault is doubled."

Had this artist availed himself of Mr. Liebreich's professional knowledge his astigmatism would have been counteracted by, say, for sketching purposes, two pairs of cylindrical spectacles; so we may presume that the whole of Mr. Liebreich's graphic account is derived solely from scrutiny of the artist's portraits.

Our wonder is that an intelligent artist should not have realised the fact that he had acquired an ocular defect which caused him to see things otherwise than he used to do, and that his mind ever rested until he had found the means of obviating it. Because, as the lecturer remarks when on another topic :—

"If in consequence of astigmatism a square appears oblong to any one, he can measure the sides with a compass; or, what is more simple still, he can turn it so that the horizontal lines are changed into vertical ones, and *vice versâ*, and his own sight will convince him of his error."

It would be diamond-shaped with its diagonal, horizontal, or vertical, he might have added. Furthermore, all the studs and buttons on the dresses of those who sit to the artist for their portraits, every coin he handles, his own watch, &c., will be ellipses or ovals whose length retain their horizontality however much they be rotated round their axes; but will cease to do so if he incline his head to either side.

An astigmatic, as long as his eyes are *fixed* upon the centre of a cross with equal limbs, may see the vertical and horizontal limbs of unequal length, and several such cases are on record. But we are not aware that these persons would have been mislead into believing that the limbs differed in length. The whole of Mr. Liebreich's illustrations involve the supposition that the artist copies from the picture formed in his own retina when he fixes his gaze upon a—say the middle—point of the object he wishes to draw. But this is just what a painter never does. He copies from no *one* retinal picture, but from the central points of *numberless* retinal pictures, to the total neglect of the whole of the eccentric parts of these pictures. Normal eyed folks do not determine the shapes and sizes of objects until they have swept the optic axes swiftly and repeatedly along their outlines and over their surfaces, and thus, through the muscular sense, have taken note of the movements of the eyeball, head, or body required to effect these purposes. The height and breadth of a man, for instance, we should measure in this way. The astigmatic is not incompetent to do the like.

He would copy no line every point of which had not been imaged in his *foramen centrale*, and draw no line towards every point of which his optic axes had not been directed. With any sort of precaution an intelligent astigmatic, even without resorting to spectacles, need not be fated to draw brachyocephalæ as dolichocephalæ; nor to turn out from a subject fitly personating Sir John Falstaff in buckram a portrait that would pass current for Don Quixote.

Again, the lecturer in another place, remarks:—"We are always conscious of *indistinct* vision, be it in consequence of incorrect accommodation. . . ." and surely the vision of this astigmatic could not be distinct! If he have on his retina the image of a single luminous point formed by rays that have travelled in the ocular meridian to which he owes his farsightedness, there will be a point in front of the image whereat those rays from the luminous point that have kept in the plane of his shortsightedness will cross one another; and between this point and the image those keeping to other meridians will intersect; and all these decussating rays will proceed to impinge upon the retina round about the image, which then becomes the brighter middle point of an oval halo. In a slightly different manner only, a halo would be formed about a like retinal image belonging to the distance of shortsightedness. From this diffusion, or aberration of rays he would plainly be troubled with very indistinct vision, and could hardly distinguish the finer details of pictures, or read the printed matter now before the reader. Hence the astigmatic could not avoid being convinced of his badness of sight. If he had not in both eyes a *precisely* similar astigmatism, some inkling as to the nature of the fault in his vision might be forced upon his attention from this source.

We would not put our experience of astigmatism in competition with the lecturer's; but such as it is, it is not so parallel to his that we should be free from misgiving were we to infer that such faults as he detects in the portraits must be entirely astigmatic and not at all mental. In the cases of astigmatism we are acquainted with, if both eyes have been affected, they have been dissimilarly so; and we know of no intelligent astigmatic who would have seen objects elongated without being aware of his liability to do so. There are many notable cases of astigmatism which are not owing to any lateral compression of the eyeball; such, for instance, as where persons ordinarily see two or even three distinguishable images of objects upon a single eye. The primary cause of which anomaly is, we believe, that the apex of the cornea and the two apices of the crystalline lens are not all pierced by the axis of the eye. We are familiar with a person who sees, within certain limits, two

images of an object with one eye, and mostly within other limits, three images with the other. Yet no one is more acute at any distance in detecting an irregularity in form, and certainly he never miscounts: like an observer with an imperfect astronomical instrument, he knows what errors of observation to correct for. By means of two *spherically* ground glasses of different focal lengths, his eyes have been rendered practically devoid of defect for his reading distance. And this can be done for the majority of eyes affected after this type.

We are now conducted by the lecturer to the anomaly of sight called colour-blindness:—"This moderate degree of colour-blindness does not always deter people from painting. A proof of this I saw in last year's exhibition, in a picture which represented a cattle-market. The roofs of the surrounding houses were all painted red on the sunny side, green in the shadow; but—which particularly struck me—the oxen also were red in the sun, green in the shadows." Whether it be true or not that there are "slighter degrees of colour-blindness," where an intense red can be perceived as such, whilst a less intense red appears green," it is a fact that shadows, cast in coloured light, provided that some white light falls also on the shadowed surfaces, are of the complementary colour; that is, if the light of the sun, as is likely when it is low enough to throw long shadows, is red, whilst, as is also likely, the sky above or clouds might nevertheless yield white light, the shadows of objects in the sun-light should be green. Every tyro in painting has been taught this law of colouring. Thus, if there really were some exaggeration, or incompleteness in indications in the picture offensive to a connoisseur, we would ascribe it to some error in judgment—as undue straining after effect. At any rate, the hypothesis that the artist was afflicted by colour-blindness cannot be indispensable.

The lecturer closes his illustration of faults of vision by the example of Mulready, whom he supposes to have painted his later pictures too blue, from a yellow discoloration of the crystalline lens very usual in advanced age, and of which the artist himself was unconscious. The description of this case is perfect; the fact from his own practice that many patients from whose eyes he had removed a crystalline lens (on account of cataract), "declared, immediately after the operation, that they saw everything blue," and his observations on the artist's pictures, viewed through yellow glass, are put in a telling manner.

However, Professor Maxwell[1] had said, just a year before, at the same institution in which Mr. Liebreich made his statements—

[1] Op. cit., p. 377.

"Experiments of colour indicate very considerable differences between the vision of different persons, all of whom are of the ordinary type. A colour, for instance, which one person will pronounce pinkish, another person will pronounce greenish. This difference, however, does not arise from any diversity in the nature of the colour sensations in different persons. It is exactly of the same kind as would be observed if one of the persons wore yellow spectacles. In fact, most of us have near the middle of the retina a yellow spot through which the rays must pass before they reach the sensitive organ. This spot appears yellow because it absorbs the rays near the line F, which are of a greenish-blue colour. My own observations of the spectrum near the line F are of very little value on this account. I am indebted to Professor Stokes for the knowledge of a method[1] by which any one may see whether he has this yellow spot."

Hence Professor Maxwell satisfied himself that he had the spot, and that it occasioned him to see as if through yellow spectacles. Let it be granted (as we daresay it may justly be) that Mr. Liebreich is correct in concluding that Mulready's later works are all done as if he painted with yellow spectacles on;—since the artist's eyes had never had Professor Stokes's, or any other test applied to them, how can he be sure that the cause was not in the *punctum aureum retinæ*, instead of an amber-coloured crystalline lens? Here is a perplexity for our pioneer in the path of diagnosing *with precision* ocular disease from the work done by painters.

In respect, however, to the liability of painters to pursue a peculiar manner of colouring because they see certain colours differently from other people, Professor Maxwell's observations do something more than corroborate Mr. Liebreich's admonitions to artists; they evince that there are two sources of error instead of one only; and though we have felt it our duty to point out how very difficult a thing it is to consummate Mr. Liebreich's self-imposed task, we are convinced that artists would do well to carefully consider what he sets forth, and that he may be right that in some instances, anomalies of sight represent what has been looked upon as a "school or taste, a manner which may be arbitrarily changed." Perhaps, too, they might well glance inwardly when they read that

"They most unwillingly concede that peculiarities of sight have anything to do with it. It seems to me sometimes as if they considered it in a certain manner a degradation of their art that it

[1] It would not be relevant to our present purpose to give details of this method. It suffices to let an image of the white moon be formed in the *punctum aureum*, and to remark that the halo (retinal) round the moon is then, and only then, yellow to satisfy ourselves on this point.

should be influenced by an organ of sense, and not depend entirely upon free choice, intelligence, imagination, and talent."

There is one thread that runs unbroken through the whole texture of the lecturer's discourse, viz., that good painting results from the accurate copying, as far as materials will permit us, by the aid of *normal* vision. In this idea he is in accordance with the views of modern artists in general, except that, as hinted above, there should be, with any of them, a tacit assumption that with artists vision is invariably normal, or at least that they are so spiritually endowed as to be almost independent of vision. It has been thought the pre-Raphaelite school originated in the progress of photography in the realisation of details, and years ago Mr. Ruskin undertook to prove, in his bold pamphlet on pre-Raphaelitism that the "mystery" of Turner and the microscopic finish of Millais were equally exponents of the one principle of faithfully painting what they saw, that virtually the said school were, if unwittingly, followers of the man of mystery. There is an *apparent* discrepancy only which proceeds, he points out, from the different ways under different circumstances in which we see things—a fact by many unrecognised. In the one case the artist looks, as it were, successively, straight at every point of a landscape he wishes to paint, and copies successively all the points he sees; in the other he does not get near enough to see details, or he does not trouble his mind about them.

"He beholds the entire scene in broad, soft masses of true gradation, and the very feebleness of his sight is in some sort an advantage to him in making him more sensible of aërial distances, and hiding from him the multitudes of circumstances which it would have been impossible for him to represent."

In other passages Mr. Ruskin[1] explains Turner's indefiniteness from his rendering his impressions of a landscape from memory, which would not embrace an infinity of details, but still as intentional. Again he writes from a little distance:—

"Try to draw a bank of grass with all its blades, or a bush with all its leaves, and you will soon perceive under what a universal law of obscurity we live, and perceive that all *distinct* drawing must be *bad* drawing, and that nothing can be right till it is unintelligible."

The strange idea that "feebleness of sight"—whatever that *indefinite* term may mean—could really have conferred a more fascinating haze on the artist's picture we regard as *excursive* or inadvertent. At all events nothing was further from the thoughts

[1] All the citations from Mr. Ruskin's writings in this paper are taken from the "Life of Turner" already mentioned. Though we do not think that any of Mr. Ruskin's writings are referred to which we had not heretofore read.

of the hero-worshipper than to infer that the artist painted to the best of his abilities, like his predecessors, but was thwarted in his efforts by the said feebleness.

It being conceded then that the aim of an artist should be to reproduce a landscape, as we do or may see it, it is worth while to consider in how many ways of seeing we may receive enjoyment from a landscape, and whether we may in this manner find a key to any of the capricious productions of an artist of genius. An hour since we were strolling up a gentle hill, whilst straight in front of us, covered by some distant trees, the sun was getting low, and lovely were the hues in the scattered clouds that were above and lateral to it; when our eyes rested for a while upon any one of them it became the centre of an exquisite picture, wherein, however, it was the only object definitely visible; the other remote clouds and the cerulean interspaces, painted on eccentric portions of the retina, were but indefinitely conspicuous, the undulating fields with their green bushes and trees on the hedges to our right or left inaccurately focussed on the lateral and less sensible parts of the retina, contributed only their colours and general forms, but, in other hues than the clouds themselves, appreciably enhancing the charms of the central picture. In this rapt view there was blurred vision of all near objects, and it was only one of the remotest that had a defined aspect. If we might venture upon such a Hibernicism we should say that the foreground of the landscape consisted of the back ground. Were this scene painted the artist would depart from his usual mode of rejecting eccentric retinal images, and would preserve to us in its entirety one of the innumerable instantaneous retinal pictures that are received in surveying such a scene.

We are not sure that there may not be a dreamy or meditative state of the mind in which the *tenour* of a scene may be absorbed without the eyes being directed to any particular point of it. At any rate where "the darkness of the scenery" is lit only by a flash of lightning, or of cannon, some such single transitory impression of it as that just spoken of would be all that could be made upon the retina. Hence for an ingenious artist with normal vision there is scope for experiments in painting. Might any of the daubs of Turner be explained by such a prank?

Can it augment an interest in a picture that subjective phenomena be imported into it by the artist? "In the same way," Mr. Ruskin says, "the blasted trunk on the left in Turner's drawing of the spot where Harold fell at the battle of Hastings, takes, where its boughs first separate, the shape of the head of an arrow; this, which is mere fancy, is imagination, as it supposes in the spectator an excited condition of feeling

dependent on the history of the spot." The arrow head, that is, was only in the mind's eye of the artist, and its introduction into the picture is accepted as a beauty.

But were he recording on canvass the festal celebration of some critical and decisive national success, might he not aver there was not one heart in all the land that was not intoxicated with joy by slyly giving us a peep of the rising moon with ambiguous horns, insomuch that—

> "Whether she had three or four"
> One "cou'd na tell"?

Did he do so we should not only have revealed to us the bewilderment the hubbub occasioned him, but the binocular instability that accompanied it.

To go on, however, from this hypothetical case to a soberer view of the accidents of sight as affecting the works of an artist, we cannot help perceiving that they must intrude themselves into paintings. We take it that a *healthy* eye must be indeed exceptional, that is strictly devoid of a *punctum aureum*, though some eyes may have the spot yellower than others. Hence we must usually see things somewhat bluer than they are; and share in some degree, however slight, Mulready's supposed defect of vision. For even if the *foramen centrale* should be free from yellowness some of this colour would be radiated upon it from the circumjacent parts, as spoken of in the experiments of the circulating candle.

Perhaps there is no more striking instance of the influence of the accidents of vision upon art—and upon language too—than the immemorial way of representing stars with rays. The rays we actually see are nothing other than projections upon the retina of the pupil, made by such light from the star as has been reflected into the eye by the eyelashes (call them ciliary rays), and under certain circumstances by the hairs of the eyebrows (call them superciliary), and, may be, at times also by the edges of the eyelids. To these being added, if the pupil be partially covered by the eyelids four stronger rays, which are partial projections of the pupil, due, as mentioned in the early part of this paper, to refraction of the conjunctival fluids lying along the eyelids (call them palpebral beams or rays, the upward one the supra-palpebral, the downward one the infra-palpebral). Thus the eye may be said to be surrounded by stelliferous appendages. The word "stellate" is defined by Johnson thus:—"pointed in the manner of a painted star." Thus, though the origin of the deception had not been precisely unravelled in the day of the great lexicographer, he was aware that painters in thus representing stars were charging their works with subjective phenomena. On old-fashioned maps of

the heavens the number of rays often vary with the magnitude of the star from two to about eight.

If we look at a lucid point lying nearer to the eye than the least distance of distinct vision, as the light of the sun (or even diffused daylight from a window) reflected by the head of a pin, or from a bead (bubble) on a glass of water or wine we are in the act of raising to the lips, this point will appear radiated, showing from eight to twenty very fine limbs. To the uninitiated the rays of the near lucid point and of the remote one would seem to arise from the same causes; but this is not so, as the rays in the instance of the near point are produced by the "stelliform" structure of the crystalline lens. In reality, this lenticular star is included more or less distinctly in each ray of the remote point, as indeed are all the entoptical contents of the eye, as well as are the colours of the solar spectrum manifested by ocular refractions. Of the strange misnomers that have got into our language as to these stars we cannot speak here. We merely append that any luminous disc subtending only a small angle at the eye, as moon, distant gaslight, or a candle-light, may show rays as well as a star.

The Turks have never been esteemed as astronomers, but in adopting the crescent moon for the national banner, the luminary really has the features they assign it, though we have a dim recollection that they sometimes associate with it a star, for which so much cannot be said.

But could it be told, the æsthetic history of the "star-spangled banner" of the United States of America would be curious. The white stars upon it tell us more about the eyes which the great nation of the new world looks out upon other worlds with, than of the worlds looked at; which for a self-reliant people may be appropriate enough. There was a time when gold stars were displayed on the standard of France. Monarchs and vassal monarchs, khedives, dukes, marshals, &c., have felt prouder with these ciliary emblems on their breasts.

It is so natural to behold the celestial and terrestial lights that enable us to see by night rayed, that it requires a *forcible* separation of the eyelids not to do so. And though in sunshine the smallness of the pupillary opening is a hinderment to some of the rays, and of course narrows them; yet should a punctiform image of the sun be formed by even an approximately spherical, whether concave or convex, surface—such as a bowl of a silver spoon, a brass knob of a door, a china basin, a wet quartz pebble on the sea-shore—we do not see a punctiform sun, but, in lieu thereof, issuing from its site, splendid rays—plentiful, slender, and long—augmented by superciliary additions if we gather the eyebrows to escape the glare, and which the most

strenuous sundering of the eyelids cannot dispel. An artist who would paint as he sees must give us this tuft of subjective rays.

Now the conventional painted stars have their rays alike and equal; whereas the rays we really see issuing from luminous points are not all alike and equal. None are more notable than the infra-, and supra-palpebral rays, and would thus be sure to attract the attention of a discriminating artist like Turner, and in reverting, as we promised to do, to the fact that he must have known much of the infra-palpebral one, we are again prompted to wonder whether he could have been tempted to make manifest what a dominant part it may play in the aspect of scenery? or even to record how the aspect was in this way affected in his own case: thus purposely impressing his physical as well as his mental individuality upon his pictures?

Would a search among his works be rewarded by the discovery of instances in which the streaks in a picture are ascending instead of descending? When reclining supinely on couch or easy chair, as was his wont in Chelsea, to enjoy scenery, he would see the supra-palpebral beam. An artist with an aldermanic paunch in keeping back his head and shoulders to balance it, preserves the horizontality of his optic axes by directing them nearer the under eyelid, and thus becomes familiarised with the supra-palpebral beam.

When Sancho's asseverations had convinced Don Quixote that an uncomely peasant girl on an ass was his Dulcinea on a palfrey, the knight on his knees thus addresses her:—". . . . Though now some wicked enchanter spreads clouds and cataracts over my eyes changing, and to them only, thy peerless beauty into that of a poor rustic." Here we have an insane man concluding that inasmuch as the graces on which he gazed seemed to him graceless, his vision could no longer be right. Yet it gradually came to pass that landscapes disfigured by vertical streakiness grew to be as replete with charms for the adoring artist as such scenes had formerly been ere such streakiness characterized them; that he copied these blemishes believing them to be scenic realities. He a sane man in the meridian of life, never dreaming under the circumstances that there had grown clouds and cataracts over his eyes. Turner may have become fatuous a while before his death, but this portion of his life excepted, we regard such a theory as the least admissible of any. Hence these speculations on the association of subjective phenomena with painting, for we hold it to be far more probable that he should venture upon occasional experiments of this sort than that the theory alluded to can hold. Be it noted, however, we advance no theory, we aim at nothing further than furnishing a few suggestions for the use of any art-critic who

may undertake to push the inquiry into Turner's peculiarities of painting to exhaustion.

The effect of a painting upon a spectator is not wholly dependent on his sense of sight. That hero of Colman the younger who inhabited lodgings over an oven, until from being "like two single gentlemen rolled into one," "his skin like a lady's loose gown hung about him," would throughout the remainder of his life, never feel that a house could be picturesque whose form or situation suggested to his mind the possibility of an oven being concealed under its bedrooms. A man who had either frozen or melted, or both one and the other, in an attic would not gaze upon a drawing of the most elegantly gabled roof without having other thoughts in his mind than architectural effect. A painter who should place the prettiest pigstye ever imagined near the front door of a peasant's cottage, though he might mean thereby to indicate the thrift of the inmates, would call up such olfactory reminiscences that he would lavish the resources of his art in vain upon the garden, and other surroundings. He would fail to render us desirous of making acquaintance with the interior.

That a fault in perspective, or a fault in the length, shape, duration, grayness, or hue of a shadow of any body; or some corresponding error in the light reflected from it, or, perchance, refracted by it, should prove offensive in a picture, presupposes that those who are offended are so educated by habit of observation, or by precept, as to detect and resent the deviation from nature's ways. An inaccurate representation of natural or artificial objects in form or colour will be about as satisfactory as an accurate one to those who are devoid of exact acquaintance with their characteristics whether general or minute.

It is only such persons as are versed, however they may have become so, in natural laws that derive pleasure from adhesion to them in the works of an artist.

The illustration of an artist's relation to the scene he is depicting, through the instrumentality of the subjective phenomena of vision, must for picture lovers in general—even though among them be reckoned connoisseurs in the art—be a perillous expedient; because, as we fear, a scientific acquaintance with such phenomena, or if it be thought better so to state it, an observational dissection of such phenomena, is a very rare accomplishment. Nevertheless, these characteristics are, if various, as a rule, so distinctive that there is nothing but this ignorance with respect to them that can by any chance lead the viewers into the mistake that extra personal objects were intended to be painted. In the ciliary and superciliary rays of the subjective star, there is imaged on the retina with more or less clearness,

not only the star of the crystalline lens but the diffractive shadows of the iris, as also of accidental ocular corpuscles encountered by them in their way to the retina; that is, shadows bordered by several alternations of dark and bright fringes such as could not have proceeded from the landscape, or what not, intended to be painted, and the introduction of such fringes along one such image to the initiated would tell that the rays were subjective. Not only do the infra- and supra-palpebral rays reveal like images in a marked manner, but singularly striking diffractive fringes top these beams, constituting multiple images of the different lucid discs that provide us ordinarily with light. Again, drops of fluid gathered round conjunctival *grit*, will cause such a light to be veiled by splendid diffractive stripes in circles or otherwise. If a painter thought well of insinuating that every spectator of a scene was feverish or wearied from delayed sleep, by thus showing that the lubricating fluids on the eyeball were drying up, he could convey his thought to those instructed as to these diffractive phenomena without detriment to the main design of his picture. Fringes above and below the moon, or sun partially veiled by clouds, in a picture of the third day of the battle of Leipsic, would indicate that the eyes of the spectator were involuntarily closing from want of sleep.

On looking from the interior of a room out into diffused daylight, if the window has a stout cross-bar in its framework painted black, this will not be *seen* black, but as covered by dancing white dots of diversified sizes, as if being salted from a dredge, a phenomenon due to the movements of the blood in the retinal and, in all probability, partially (in another way), to movements of blood in the choroidal vessels. Black spots on white surfaces, holes or crevices in illuminated surfaces, or shadows thrown on such surfaces, have a like appearance, and an artist who makes such dark areas uniformly black instead of dotted gray will render them as no one ever actually sees them. When there is of a dark night so much mist in the air that not a star can be seen, their presence is so well simulated by the retinal movements alluded to, that it is often bewildering to discern whether any stars are visible through the mist or not; that is, the celestial or aërial canopy at night never wears a uniform aspect on account of certain subjective phenomena. In a room lit at night solely by a weak diffused light through a window, bright objects may be discernible, whilst darker ones will be concealed by a play of white dots or of sinuous white patches. A painter might avail himself of such phenomena to express degrees of illumination.

If a room be lit by light that finds its way through a metallic blind with small perforations in it, on looking out through these

holes the sky and all bright objects in the field of view will appear to be covered with black dots or lines, which are the *shadows* of the retinal capillary vessels, as the slightest oscillation of the eyes is sufficient to produce this appearance. An artist might avail himself of this fact to excite sympathy in behalf of a Galileo, or any other unworthily treated prisoner. The phenomenon would vary with the shape and size of the perforations.

Whenever a pencil of light *moves* over the excentrical portions of the retina, rays radiating from its successively illuminated points (as mentioned in the early portion of this review) towards other parts of the retina will throw thereon the shadow of any retinal vessel they encounter. Thus, if we walk along by a quickset fence through which the sun shines, the shadows of these vessels incessantly flit before the eyes. In a similar way, a light in movement at the side of the face, a flickering flame, or rays of the sun reflected at the side by machinery in motion, or an oscillating tremor, as of a wave or something swimming on such wave, might develop a like effect. Hence, an artist might intimate in this fashion either that he was in movement himself with respect to objects near him, which were not embraced by his picture, or might intimate thus that there was a general whirl and unsteadiness of all things about him.

A traveller by rail who keeps his eyes fixed upon a single object sees all others in the landscape circulating round it, because luminous retinal impressions have a transitory life. Might any painter dare to import this effect into a kind of picture we have already described as conceivable?

We could give other illustrations of the use to which subjective phenomena of vision might be turned by a speculative artist, but we will let this suffice, lest any artist should prove weak enough to be tempted by us, for the sake of novelty, to enter upon what he would undoubtedly find to be a thorny career.

II.—The Surgery of the Rectum.[1]

THE anatomical peculiarities of the rectum afford the key to

[1] 1. *The Physical Exploration of the Rectum: with an Appendix on the Ligation of the Hæmorrhoidal Tumours.* By WILLIAM BODENHAMER, A.M., M.D. Illustrated by numerous drawings. New York, 1870.

2. *Lectures upon Diseases of the Rectum, delivered at the Bellevue Hospital;* Session 1869—70. By W. H. VAN BUREN, A.M., M.D. New York, 1870.

3. *Prolapsus Fistula in Ano, and other Diseases of the Rectum: their pathology and treatment.* By T. J. ASHTON. Third Edition. London, 1870.

4. *The Surgery of the Rectum, being the Lettsomian Lectures on Surgery delivered before the Medical Society of London.* By H. SMITH. Third Edition. London, 1871.

5. *Fistula, Hæmorrhoids, Painful Ulcer, Stricture, Prolapsus and other Diseases of the Rectum.* By WILLIAM ALLINGHAM. London, 1871.

its pathology and to its special surgery. At what point the pathology of the intestinal canal passes from the province of the physician to that of the surgeon is a question very difficult to logically answer; if it can be answered. Professor Quain nearly twenty years since by his exposition of the anatomy of the terminal portion of that canal laid the foundation of a scientific treatment of its diseases. Mr. Quain said (p. 32, 'Diseases of the Rectum,' 2nd edition):

"The rectum is largely supplied with blood. The vessels as they are seen on its outer side are large, and they send branches at intervals through the muscular coat, which ramify between it and the mucous membrane. Independently of their position as regards the coats of the bowel, the arrangement is not the same throughout the rectum. Over the greater part the arteries and veins, taking both systems of vessels as following the same course, penetrate the muscular coat at short intervals, and transverse directions, and form a network by their communications with subdivisions of other similar vessels. Towards the lower end of the bowel, for the length of about five inches, the arrangement is very different; here the vessels have considerable length, and their direction is longitudinal; penetrating at different heights, they are directed in parallel lines towards the end of the gut. In their progress downwards they communicate with one another at intervals, and they are still more freely connected near the orifice of the bowel. In this place the arteries all join by transverse branches of good size. The veins form loops, and inosculate with equal freedom."

Now in respect of the most common of all the diseases of the rectum, the anatomy of the vessels is the most important element in the problem. In the treatment of hæmorrhoids it is the system of the vertical parallel arteries of Quain that constitutes the danger, and its management the means of success. Hæmorrhoids, nothing more nor less than hypertrophied, and often inflamed portions of the mucous membrane of the lowest part of the rectum, are fed by these vertical parallel arteries; no wonder, then, that simple clipping off of the protuberances thus formed *with the subsequent retraction of the stumps within the sphincter*, should have been followed by fatal hæmorrhage. Mr. Quain quotes from Sir Astley Cooper a warning example (p. 47, op. Cit.):

"Sir A. Cooper says, respecting one of the cases (a nobleman æt. 74), 'As I was anxious about this patient, I did not immediately quit the room after the operation, but stood chatting with him for a short time, when he said, 'I believe you must quit the room, for I must have a motion.' I went out of the room, and upon returning shortly after, I found him trying to get into bed; and upon looking in the vessel, I perceived a considerable quantity of blood in it. In a few minutes after he said he must have another motion, got out of bed, and

again discharged a considerable quantity of blood. This he did four different times; one of the hæmorrhoidal arteries in the centre of one of the piles which had been removed was divided; and as I was determined he should not die of hæmorrhage, I said I must secure the vessel which bled, and with a speculum ani I opened the rectum sufficiently to see the blood-vessel, took it up with a tenaculum, and put a ligature around it.' The patient, however, became gradually worse, and died in four days."—'Lectures,' &c., 12mo, 3rd Edit., p. 422.

While we had Mr. Quain's description of the vascular system of the rectum fresh in our mind it happened that we had the opportunity of examining in the dissecting room the vessels of the rectum in a case of aggravated internal hæmorrhoids; we compared the sizes of the arteries of the rectum with the sizes of different arteries in other subjects; the comparison showed that the size of the superior hæmorrhoidal artery of the subject under dissection equalled that of the brachial of a neighbouring *female* subject. Where then is the ground for surprise at the persistence or the profuseness of bleeding from the rectum in long standing disease? The superior hæmorrhoidal is the direct feeder of the vertical arteries, and is in quite as close relation to the vertical arteries as the brachial is to the palmar arches; and the trouble a wound of one of the palmar arches often entails is too well known.

But the hæmorrhoidal veins present certain peculiar features; Mr. Curling thus describes them in his work on 'Diseases of the Rectum,' p. 28:

"The hæmorrhoidal veins distributed in the submucous tissue at the lower part of the rectum communicate in loops, and form a plexus which surrounds the bowel just within the internal sphincter. The veins are best seen when somewhat congested, their deep purple hue being very apparent through the thin mucous membrane with which they are in close contact. The plexus is then seen to be about three quarters of an inch in length, and composed of veins of various sizes, arranged the most part lengthwise and in clusters, being especially collected in the longitudinal folds of the rectum. The plexus does not extend lower than the external sphincter, but branches from it, passing between the fibres of the internal sphincter, descend along the outer edge of the former muscle, close to the integument surrounding the anus.''

Plate IV of Mr. Quain's illustrations affords a view of these veins, and of their relation to the sphincter, while Plate II gives the terminal disposition of the arterioles in their relations with the veins.

The sphincter ani also plays a prominent *rôle* in many forms of rectal disease, thus in internal hæmorrhoids, the thickened and sensitive mucous membrane extruded during the act of defæcation is straightway strangulated by the spasmodic

contraction of this circular or almost circular muscle. The nerve supply of the sphincter is by no means to be ignored, for in operations for the cure of hæmorrhoids, the claims of the hæmorrhoidal nerve to consideration cannot be overlooked. There is the nervous sympathy of the perineal surface to be consulted as well as the nervous endowment of the actual muscle itself.

Although Mr. Allingham's hospital statistics apparently show fistula to be the most common disease of the rectum, there can be little doubt that some form of hæmorrhoids is really far more frequent; there are, we suspect, scores of persons who suffer, and suffer severely, from hæmorrhoids, and allow their troubles to drag on, for one that may not think it worth the while to apply for medical relief for a fistula. We will accordingly examine our different authors on the treatment of hæmorrhoids. In the treatment of hæmorrhoids we have to deal, firstly, with the thickened and inflamed portion of the mucous membrane of the rectum and with the enclosed venous plexus; secondly, with the large arterial supply from Quain's parallel arteries from above; thirdly, with the muco-cutaneous membrane of the anus liberally endowed by branches of the pudic nerve, continuous below with the skin of the perinæum. What line of action can be clearer; we must remove the excrescence, prevent hæmorrhage, and obviate nervous irritation and preventible pain; and avoid risk of phlebitis and of pyæmia.

Dr. van Buren says (p. 35, et seq.):

"The means which have been employed to destroy the tumours are various; I have tried them all except excision, and can confidently recommend to you *strangulation by the ligature* as the safest, surest, and most manageable procedure.

"The use of the knife or scissors was fully demonstrated by Dupuytren's experience to be dangerous; he lost several cases from hæmorrhage, which comes on insidiously after the operation—the blood not escaping externally, but accumulating gradually in the cavity of the bowel. The actual cautery is a repulsive procedure, and not easily applicable under all circumstances. Galvano-cautery promises well, when proper apparatus is at hand. Of the potential caustics, nitric acid acts too slowly; and the others, in addition to this objection, are unmanageable. Chassaignac's *écraseur*, and its modifications, in which iron or copper-wire is substituted for the chain, require more time in their application, and bleeding *does* sometimes follow their use in this operation. The various clamps recommended by the instrument makers are, to say the least of them, unnecessary; and injection of the tumours with solution of persulphate of iron is painful and inefficient. On the other hand, a stout ligature of silk, or gut, or hempen thread, is always to be readily obtained; its application requires no great amount of anatomical or surgical skill; and the result you will find certain and satisfactory—if you follow the rules I am about to give you.

"The patient being in good condition for operation, with bowels acting regularly and well, let him delay his daily stool until your visit, and present himself to you immediately afterward, with his piles thoroughly protruded; let him stand, bending forward over a bed or chair, with the parts exposed to a good light. Having provided yourself with a tenaculum, a double hook—such as is found in every operating-case, forceps, scissors, and several stout needles armed with long double ligatures, seize the largest of the tumours with your hook—which you transfer to an assistant, telling him to draw gently upon it; then pass a curved needle pretty deeply through the base of the tumour, draw it through to the middle of the double ligature, cut the needle free, and proceed to tie one of the ligatures as deeply as possible, at either side of the base of the tumour, drawing your first knot tightly, so as to strangulate the included tissues thoroughly. Repeat this procedure upon each of the remaining tumours—there are rarely more than three or four at the most, sometimes only one or two—cut off your ligatures short, and then carefully replace the strangulated tumours within the cavity of the bowel. This is the outline of the operation; now for the details. If your patient cannot get his bowels to act at the time of your visit, or if the tumours do not come down satisfactorily, let him have an emena of tepid water, and try again. If they tend to retract during the operation, let him sit over warm water and strain; and it is well to have a curved spatula, or Sims's speculum at hand; also, to transfix and thus secure all the tumours you propose to ligate, before you begin to tie. Introduce your curved needle from without inward, protecting the gut from its point by your finger; strive to get well up into the bowel, and, if possible, avoid including any of the delicate semi-mucous integument of the anus in your ligatures, as this increases greatly the pain of the operation at the moment, and afterwards. If you are successful in this, the pain of the operation is really trifling. If you cannot succeed to your satisfaction, it is better to divide the integument on the anal side of the tumours' base by the knife or scissors, and, in tying, lodge your ligature in the groove thus made. This is a practical point of importance, for the delicate semi-mucous membrane of that portion of the rectum habitually grasped by the sphincter is far more sensitive to violence than the gut within; and, when included in a ligature, it is painfully pinched by the irritated muscle, becomes œdematous and rolls ont at the anus, giving the patient the unpleasant idea that his piles have come down again. Moreover, like one of the varieties of external hæmorrhoid, this sort of swelling is very slow to disappear, and then leaves behind it a tab of loose skin.

"In the majority of cases requiring this operation your patient will claim the benefit of anæsthesia, or, if of the other sex, it will become you to recommend it, so as to spare her modesty, as well as to prevent possible pain. Here, not having the voluntary assistance of the patient in forcing down the hæmorrhoids and presenting them for operation, you will be obliged to vary your mode of procedure very materially, or you will operate at a disadvantage.

"I have adopted, under these circumstances, the following mode of managing the patient, and it has succeeded so well in my experience—which covers now a large number of cases—that I can safely recommend it. I have found it of little use to have the patient force down his piles before the anæsthetic is administered, inasmuch as they are very apt to slip into the bowel again as the sphincter becomes relaxed; but I prefer that the patient should rinse out the bowel by an enema of tepid water, before he takes his place upon the couch or table. This latter should be firm, narrow, of convenient height, and in a good light. As soon as the patient is fully under the influence of the anæsthetic, I have him placed in Sims's position for operation on the uterus and vagina, that is, with the upper part of the body prone, the hips elevated, and the thighs flexed on the abdomen. There should be an assistant to take entire charge of the administration of the anæsthetic, and at least one more to aid the operator.

"I then commence the operation by thorough and complete *forcible dilatation* of the sphincter-ani muscle, by which the interior of the lower part of the rectum is placed entirely at my disposition, and afterward proceed to the ligature of the hæmorhoidal tumours in the manner and with the precautions already described. The paralysis, or to speak more accurately, the atony, of the sphincter muscle, which results from this manœuvre of forcible dilatation—which I will explain more fully another time—secures not only the great advantage to the surgeon of free and ready access to the lower part of the rectum, but it saves pain and trouble to the patient after the operation. The muscle does not recover the full vigor of its contractile power for a week if the manœuvre has been thoroughly accomplished, and meanwhile the patient is spared much pinching of tender parts. I have thought that retention of urine, which sometimes follows the operation for internal piles, has been prevented by it.

"As to the question of danger of the operation by ligature: in selected cases, it is so trifling as to be hardly appreciable. In between sixty and seventy operations, I have never had an unpleasant result. In over one hundred cases of operation by ligature, the late Valentine Mott had one fatal result—in a gentleman "who had just before met with great reverses in business." From the symptoms as recounted, I infer that the cause of death, in this case, was suppurative phlebitis, as he "fell into a typhoid condition" shortly after the operation, and "small abscesses were found in the liver after death." I have knowledge of one other case where a similar result followed a partial operation; the patient, a medical man, having subjected himself to exposure and over-fatigue within a day or two afterwards."

Dr. Bodenhamer uses "almost exclusively" a silk ligature; he declares

"It is seldom that my patients, during the whole course of treatment, are ever confined for a moment to either their rooms or their

beds, but are enabled at all times to be up and attend to ordinary business" (p. 53).

"The old method of tying piles, that which is recommended in the books taught in the schools, and usually practised, is (after the bowels have been evacuated and the tumours protruded as much as possible) to seize each tumour by either tenaculum or forceps, and draw it down fully out of the anus, and apply closely to the base of the part thus drawn down, a strong heavy silk or hempen cord, and then the same drawn and tied as tightly as can be; or a curved needle armed with a double ligature, is passed through the base of the tumour, so as to divide it into two, and the cords tied as tightly as possible on each side. After the tumours are all tied, they are returned within the anus, and an enema of starch and laudanum administered, the patient required in the meantime to maintain the horizontal posture, to live on meagre diet, and to avoid having any fæcal evacuation for six or eight days. Sometimes immediately tying the tumours, they are then amputated closely to the ligature. This, in short, is the usual process now practised in the ligation of hæmorrhoidal tumours, and from the very nature of the case, must necessarily be attended with more or less danger, and with severe pain and inconvenience.

"The great objection to this method of operating is, the extreme suffering which follows and continues for a considerable time, and the confinement to either bed or room for several days. It is said, too, by some authorities, that the operation is attended with great danger from *tetanus*, *phlebitis* or *pyæmia*, &c. This danger, however, has been, and is, in my opinion, greatly exaggerated. The few fatal cases reported were never verified by a *post-mortem* examination, and consequently are deserving of but little confidence. I believe when danger, extreme pain or failure attends the operation, it is generally referable to the unsuitableness of the ligature, and the injudicious manner in which it has been placed upon the tumour. I have, by my peculiar method, operated in thousands of instances, and have yet to encounter the first serious accident.

"The circumstance, then, of the danger, pain, and inconveniences attending the old operation, led me, about twenty-five years ago, to seek for information in relation to it, with a view if possible to remove some of the obnoxious features of it, or so to modify it as to make it less objectionable and serious; without at the same time rendering it any less efficacious in the cure of the disease. I first began by making some experiments upon both internal and external hæmorrhoidal tumours, when in a quiescent state, expressly with a view to ascertain whether any one point or portion of the tumour was more sensitive than another; and more especially whether the mucous membrane, or other tissues from which such tumour proceeded, was more or less sensitive than the tumour itself or its covering. The experiments were conducted by means of a peculiar forceps, expressly made for the purpose. The apex of the tumour was first seized by the instrument, and firm compression made; the middle portion next, then the base, and lastly a portion of the lining

membrane of the rectum, to which the tumour was attached, was included in the blades of the forceps. I found that in proportion as the compression reached the base of the tumour the pain was increased, and when a portion of the mucous membrane of the rectum or other tissue was included in the blades of the forceps, the pain was very severe. I have ever since, in operating, been very careful so to adjust the ligature as not to tie it too close to the base, and that nothing but the tumour itself should be included in its grasp. Now the question naturally arises, what is the cause of this difference in the sensibility of the natural textures from which the tumour proceeds, and those of the tumour or foreign growth itself? The most rational inference which occurs to my mind at present is, that the former are more abundantly supplied with nerves and nervous influence than the latter. Be this as it may, however, the fact is as I have stated it, and it is in the power of any student to verify it. But on this, as on many other points of pathology and physiology, we are sometimes much better acquainted with the *quo* than with the *quomodo;* in other words, we know the facts, but we cannot well explain them. My researches on this subject have plainly taught me that so far as the natural tissues are concerned, the fine and delicate skin immediately without the anal orifice is the most sensitive; that the muco-cutaneous coat immediately within the anal orifice is next in point of sensibility, and that the mucous membrane of the rectum is the least sensitive of the three. I, however, have found the mucous membrane of the rectum much more sensitive than the mucous membrane covering the tumour. This must not be forgotten. Indeed, the foreign body and its covering, unless entirely external and covered with true skin, are much less sensitive than the three natural textures previously named.

"I now propose to offer some improvements in the operation of ligating hæmorrhoidal tumours, the success of which has been invariable, and warranted by an experience of twenty-five years.

"By my method of operating, the tumour to be ligated is never seized by tenaculum nor forceps, and pulled down; for if this is done, a portion of the elastic mucous membrane of the rectum, to which the tumour adheres, also comes down with it, and therefore is almost certain to be included in the grasp of the ligature—hence the additional pain and suffering which necessarily follow; for the operator cannot distinguish the true base of the tumour from any other part when drawn down in this manner, for all the parts generally have the same appearance. I always require my patients to extrude the tumours simply by defecating efforts, or by the efforts produced by means of an aperient or a relaxing enema. If one or all these means should fail to protrude the tumours, I employ a bivalve speculum ani, introducing and arranging it in such a manner that the tumour which I design to ligate should fall between its blades; then, with suitable instruments, it can be ligated within the canal, just as easily as if it were extruded or external. I scarcely ever take up more than one tumour at one time, and never employ a heavy silk cord with a hard twist in it, such as saddler's silk, which

is the article often used for this purpose, but use a fine silk ligature, well waxed, with scarcely any twist in it, somewhat like floss or dentist's silk; for in proportion to the size of the ligature and the hard twist in it, will be the increased pain it will occasion, and the length of time it will take the tumour to slough off. As before observed, I am careful so to adjust the ligature as to exclude everything but the foreign body itself, and only make the ligature sufficiently tight to cut off the circulation—nothing more nor less. This can be known and adjudged by the appearance of the tumour whilst the ligature is being tightened. I am also careful not to place the ligature very close to the base of the tumour, as this produces more pain, and is not any more effectual in removing the whole of it. The small portion of the base of the tumour being below the ligature, will also sooner or later completely slough off. When the tumour is very large, or too large for one ligature, I divide it into two or more sections, according to its size, and multiply the ligatures, including but a small portion of the tumour in each. This is done by arming a suitably curved needle with a double ligature, passing it through the base of the tumour, aud, if necessary, repassing it, and trying each ligature separately—thus including in the stitches every part of the tumour, and underlaying it, as it were, with a double uninterrupted suture. When part of the tumour is covered with true skin, or muco-cutaneous tissue, I usually incise this upon the same circle which is to receive the ligature afterwards, by which more or less suffering is avoided. I sometimes, when the tumour is entirely external and covered with true skin, and objection made to the knife or curved scissors, ligate it subcutaneously, which causes it to shrivel and gradually to disappear."

Mr. Smith, on the other hand, although he admits (p. 97) that the application of the ligature has proved to be admirable and permanent in its results, ardently advocates the use of the clamp, for as regards the ligature—

"It was found that there were many disadvantages connected with it, and that it would be desirable if some other means of treatment equally efficacious could be put in force."

"About thirty years ago Mr. Cusack suggested the employment of the clamp and cautery as a means of destroying hæmorrhoidal tumours, and his practice was followed by other surgeons in Dublin. In this country Mr. Henry Lee adopted the method of using the clamp, cutting off the tumours, and then stopping the hæmorrhage with the actual cautery. The results of this gentleman's experience induced me to put the plan in force, and in the first cases which occurred in my practice I was so satisfied with the treatment, and so convinced of its superiority over the ligature, as regards the important elements of safety to life, freedom from suffering, and saving of time, that I determined to treat in this method those cases, both of hæmorrhoids and prolapsus, where I thought the ligature was inadmissable, or where the patient objected to it, as well as those

wherein the application of nitric acid alone would not suffice to bring about a cure.

"I was not long, however, in finding out that the clamps hitherto used were capable of being greatly improved, for these were either of an awkward shape, were so constructed as regards the apposition of their edges, and so totally unfurnished with regulating power, that the efficiency of the instrument and value of the treatment were much impaired; so I suggested to Mr. Matthews to make me an instrument shaped somewhat like the ordinary clamp Mr. Curling uses for applying nitric acid; but the edges, instead of being serrated, or not meeting in their entire length, as in an instrument used by Mr. Henry Lee, were to be so constructed that they accurately fitted their whole extent by means of a groove on one blade and a raised surface on the other. I soon found the value of this, for the mucous membrane or tumour to be removed could be thoroughly compressed, and there was no fear of the bleeding which would take place when the clamps with serrated edges were used and the tumour excised. I next improved the instrument by furnishing it with a catch, and by this I was greatly assisted; but when the pressure was taken off the divided base of the tumour the cut surface would suddenly recede from the grasp of the blades, and if some vessel not thoroughly cauterised should bleed, it was difficult to get hold of the part again; so, to meet this emergency, I had a light but powerful screw added to the handles of the instrument, by means of which I could so regulate the power of the blades as to take the pressure off the cut surface of the rectum gradually, instead of suddenly, so that if any portion of the divided surface was not thoroughly cauterized the bleeding point would show itself, although still retained within the grasp of the blades, and by the slightest turn of the screw it could be secured and the bleeding vessel be entirely sealed up. This addition to the instrument I consider to be of the utmost importance, and, indeed, I never think of performing an operation without employing the instrument furnished with the screw.

"The operation, whether for hæmorrhoids or prolapsus, is very simple, and consists of the following manœuvre:—The diseased portions, being well brought down previously by an injection, are separately seized with a vulsellum and handed to an assistant. The part is then enclosed within the blades of the clamp, which are screwed home quickly and thoroughly; the prominent portion of the pile or prolapsus is then cut away by a sharp pair of scissors, the cut surface is next dried by a piece of lint or sponge, and either the strong nitric acid or the actual cautery, so shaped as to come into contact with the whole of the raw tissue, is applied; when this is effected the blades are gently and slowly unscrewed, and if there is no bleeding the part is well oiled and allowed to return within the cavity of the gut; if, however, any bleeding point is seen, the blades are quickly screwed together, and the cautery is applied until the vessel be thoroughly sealed up. The finger is then introduced well up into the rectum. This step serves the triple purpose of returning all the parts well, of compressing any point which might possibly bleed, and

of exciting the sphincter to healthy action. I generally introduce at the same time a suppository of opium.

"Now, it might appear to some that this operation would be very painful, but, singular to relate, if great care be taken not to include any of the integument within the blades, and not to allow the nitric acid or cautery to come into contact with it, the patient does not feel much pain, and really does not know when the heated iron is being applied. If, however, the cautery be kept in contact with the blades of the clamp for more than a few moments, the patient will suffer pain by the transmission of heat through the contact of the metallic surfaces, and therefore it has been suggested to me to have some non-conducting medium applied to the under part of the blades. It is possible that I may make some alteration in this presently; but a grave objection lies in the fact that by this means the size and thickness of the blades would be materially increased."

Mr. Allingham (p. 89) describes the mode of treating hæmorrhoids by the ligature as applied at St. Mark's Hospital, after the manner devised by the late Mr. Salmon. Expressing his opinion that ligature is by far the best and most generally applicable method of operating upon hæmorrhoidal diseases, he guards against being understood as referring to "the usual method of applying the ligature by transfixion of the base of the pile and tying it in halves." The operation he recommends is as follows:

"The hæmorrhoids are to be seized by the operator one after another with a vulsellum or pronged hook-fork, and drawn down; he then with a pair of strong sharp spring scissors separates the pile from its connection with the muscular and sub-mucous tissues upon which it rests; the cut is to be made in the sulcus or white mark which is seen where the skin meets the mucous membrane, and this incision is to be carried up the bowel, and parallel to it, to such a distance that the pile is left, connected by an isthmus of vessels and mucous membrane only.

"There is no danger in making this incision, because all the larger vessels come from above, running parallel with the bowel, *just beneath the mucous membrane*, and thus enter the *upper part* of the pile. A well-waxed, strong, silk ligature is now to be placed at the bottom of the deep groove you have made, and the assistant then drawing out the pile with some decision, the ligature is tied high up at the neck of the tumour as tightly as possible. If this be done, *all the vessels must* be included. The silk should be so strong that you cannot break it by fair pulling. A portion of the pile may now be cut off, taking care to leave sufficient stump between the ligature to guard against its slipping. When all the hæmorrhoids are thus tied, they should be *returned thoroughly* within the sphincter; after this is done, any superabundant skin which remains apparent may be cut off; but this should not be too freely excised for fear of contraction on the healing. An injection of Liq. Opii sedativus may be

administered. I always place a pad of wool over the anus, and a tight T-bandage, as it relieves pain most materially."

One very important practical point Mr. Allingham adds, a little further on (p. 95).

"I am quite convinced that the higher you carry your incision up the bowel the less does the patient suffer, because the ligatures are removed from the most sensitive part of the rectum and lie quietly above the sphincters."

In opposition to Dr. Bodenhamer as quoted, Mr. Allingham urges rest after the operation. He says (p. 92):

"I think it advisable, though not absolutely necessary, that the patient should keep lying down until the ligatures separate, which almost invariably takes place about the sixth or seventh day, occasionally a day sooner, very rarely a day later. If the ligatures are tied tightly and the incision has been free, this course of events is but very seldom departed from. *Active exertion*, even after the separation of the ligatures, is to be deprecated until the sores left in the rectum are quite healed; a fortnight or a little longer is generally about the time required to accomplish this. It is quite unnecessary that the patient should be kept in bed all this time, or even to his chamber—he may move about in moderation; but I am quite certain that a too speedy resumption of the erect position is likely to retard the cicatrization of the wounds."

Under five heads, advantages are claimed by Mr. Allingham for Mr. Salmon's operation as performed at St. Mark's Hospital:

"1st. The rapidity with which it may be executed. I have often operated upon four or five hæmorrhoids, returned them, and removed redundant skin in one minute and a half or two minutes.

"2nd. There is only a very small amount of tissue included in the ligature; in fact, little more than the vessels supplying the tumour.

"3rd. At least three quarters of the wound is a simple incised wound which heals rapidly, only the small portion included in the ligature having to slough away.

"4th. The ligatures are tied a considerable distance from the anus, so that, when returned into the bowel, they lie above the internal sphincter, where the sensibility of the mucous membrane is not acute, and consequently the pain and irritation after the operation is reduced to a minimum.

"5th. The operation is wonderfully free from danger to life, and its results generally are almost always satisfactory."

Mr. Allingham combats Mr. Smith's statements in praise of the clamp and cautery, which he stigmatises as "extravagant," and declares that he has seen a much greater proportion of un-

toward results from the clamp than from ligature, not only in his own practice, but in that of other surgeons; that the suffering after the operation is very considerable, the patient not being more free from spasm of the sphincter and retraction of the levator ani than after ligature; and that a good many times he had been annoyed after the clamp operation, to find that his patient had slight but recurring arterial hæmorrhage.

The clamp, then, would appear to be admissible in those cases of internal hæmorrhoids where the tumours are not numerous, and where especially there are no external piles or hypertrophic skin requiring removal. That the ligature is the safest and most effectual remedy, the opinion expressed by Mr. Curling in 1863, is thus supported by subsequent testimony.

The clamp has another antagonist in Mr. Ashton, who declines to admit its advantages; he further adds that there is

"The impossibility of including between the two straight lines formed by the jaws of the instrument, so irregular a growth as that constituting the base of a hæmorrhoidal tumour, and which is also frequently attached as high as the upper margin of the internal sphincter." P. 121.

Mr. Ashton figures curved needles, having a cutting edge on the concave border, and furnished with two eyes, so that after the section of the anal attachment of the pile, the needle being driven throught its base from without inwards, both eyes may be threaded with "ligatures of gold thread made of longitudinal strands of silk bound round by a spiral band of fine gold." On the withdrawal of the needle, the two ligatures occupy its place, and can be tied, one on each half of the tumour transfixed. Mr Ashton strongly impresses the necessity of drawing the ligatures as tightly as possible and of including the whole of the tissues affected, differing from Mr. Curling, who states that the contraction of the cicatrix is sufficient to reduce any part that may have escaped the ligature. Mr. Ashton thus follows the practice of Mr. Quain at University College Hospital (op. cit., p. 44). Mr. Ashton's needles probably afford a convenient instrument for landing the double ligature.

If from this common affection of the rectum, which fortunately, proves so amenable to treatment, we turn to rarer diseases—obstructions of the rectum due to stricture and to cancer—we shall meet with questions of great interest in pathology and surgery. We have long had the conviction that strictures of the rectum, not cancerous, are for the most part syphilitic; and although it has not been possible for us in every case of stricture of the rectum in women to show signs of constitutional syphilis

on the surface, nevertheless the patient always has been a person likely to have suffered from syphilis, whenever palpable signs of syphilis did not exist. A patient, for example, to all outward appearance healthy, has stricture of the rectum: the history of her past life will show that she has run the risk of syphilis; and there will probably be that condition of the margin of the anus justifying the suspicion of mucous tubercles and of fissures at a former period. On the other hand, we have had in some patients syphilodermatous phenomena that have rendered denial of previous chancre superfluous—cases where we could point to palmar psoriasis, loss of hair, spots on the scalp, and cracks at the angles of the mouth. But constitutional syphilis presenting palmar psoriasis, &c. &c., is only too common, whereas stricture of the rectum is comparatively a rare disease; the missing link in the chain of causation, the initial symptom, is, we think, overlooked; it consists, we believe, in catarrh of the rectum. The following case fell under our own observation:—A lady during her first pregnancy by her husband, *who was at that date of his marriage in the early stage of constitutional syphilis*, had some symptoms of syphilitic infection; she miscarried at the seventh month; she afterwards suffered from catarrh of the rectum, with fissure of the anus and vaginitis.

Why should not catarrh of the rectum be followed by stricture of the rectum, just as urethritis is followed by stricture of the urethra? When a stricture of the rectum in a female exists, why need we search for the cicatrix of an ulcer, syphilitic or dysenteric, or for any extraordinary exciting cause to explain the fact? Is it not probable that the close relationship of the vagina and rectum has an important bearing upon stricture of the latter organ? But in how many cases would not catarrh of the rectum be considered by the patient as a mere irritation of the lower bowel, and be disregarded as one of the inevitable ills connected with child-bearing, or rather, with miscarriage!

It is not a little remarkable that the majority—a large majority—of cases of non-cancerous stricture of the rectum consists of female patients. Mr. Curling says that out of twenty-eight cases, twenty were women (op. cit., p. 117). Mr. Curling mentions "an instance of a girl, aged thirteen, who had suffered from stricture in the rectum quite four years, consequently since the age of nine." This is the earliest age at which he had met with the complaint (op. cit., p. 124). We have ourselves met with a female child under that age suffering from stricture of the rectum that was clearly the consequence of congenital syphilis.

Dr. van Buren (p. 109) thus declares his opinion, an opinion in direct contradiction to our own:

'I mean to say that stricture of the rectum is not one of the recognised manifestations of constitutional syphilis; and I make the statement in this form because the contrary opinion was at one time quite prevalent among the older surgeons. This doctrine culminated about the period of Desault and John Hunter, at which time mercurial treatment was pretty generally employed in the treatment of rectal contractions. Since clearer ideas have begun to prevail as to what syphilis really is, and the distinction between it and other contagious sores contracted in promiscuous intercourse, known as simple or soft chancres, is more generally recognised, it has become evident that true syphilis has little or nothing to do with the causes of stricture of the rectum."

After adducing the evidence of Gosselin, Després, F. Probst, Van Buren concludes, that

"In summing up what is certainly known on this subject we may conclude that there is no form of the disease to which the name of *syphilitic stricture of the rectum* can properly be given, and that, although gummy deposit may possibly occur in this locality as elsewhere, and fall into ulceration, it is probably of very rare occurrence as a cause of stricture; and that stricture, as such, is not amenable to anti-syphilitic medication."

Does one's clinical experience in this country justify anything such as suggested by Dr. van Buren? (p. 109):

"Thus phagedæna, which is so liable to complicate non-syphilitic or soft chancre, is a recognised cause of stricture of the rectum, but rarely encountered, except in women of a certain class, where chancrous pus, flowing from the vagina, has inoculated abrasions at the anus, and the ulcers then produced have assumed the phagedenic character, and extended within the bowel."

We would much rather endorse Mr. Smith's opinion that

"It is extremely important to recognise the syphilitic poison as a cause of the disease; otherwise we shall be led into the error of limiting our treatment to local measures alone, whereas constitutional treatment for the specific affection originally causing the stricture must be employed at the same time" (p. 42).

But we entirely disagree with Mr. Smith when he attributes the origin of stricture to syphilitic ulceration *of the parts in the neighbourhood* of the lower portion of the gut; and, indeed, he admits that the stricture is occasionally found at the upper part of the rectum. However, when abscesses and fistulæ have formed by ulceration extending from the gut *above the seat of stricture*, what course is to be pursued? Every surgeon who has had any experience knows that the ordinary "cutting for fistula"

is worse than useless. This question brings us face to face with the operation of colotomy in the lumbar region (Amussat's operation). In the management of cases of cancer of the rectum colotomy is a resource of the greatest value; this operation it is our duty to propose to the patient if there be no obviously contra-indicating circumstances. The archives of surgery contain sufficiently numerous examples of lives temporarily saved, and comfort afforded, to render it imperative, where communication exists between the bowel and bladder, or between the bowel and vagina, and where constant distress renders life an insupportable burden, that the surgeon should firmly recommend colotomy. When complete obstruction exists, delay tends so much to diminish the chances of a favorable result, that the expediency of the operation before inflammation has been set up and the intestines have been damaged by over-distension must be authoritatively declared.

The danger attending the operation of colotomy, Mr. Curling says, is much less than is commonly supposed (p. 169, op. cit.). Mr. Pollock (article on "Diseases of the Alimentary Canal," Holmes, p. 180, vol. iv) thus describes the operation:

"The incision should commence in front of the ongissimus dorsi, and be carried forward to the extent of about six inches. The integuments having been divided, the muscles are to be carefully cut through until the intestine is exposed. This is not a difficult or troublesome proceeding The bowel should be at once hooked up by a curved needle; two or more points should then be secured by threads to the margin of the wound, and the gut opened."

Mr. Allingham (p. 224) has found, from numerous dissections, that the descending colon is always normally situated half an inch posterior to the centre of the crest of the ilium, and he before operating is in the habit of marking this spot either by a piece of adhesive plaster or a touch of strong iodine paint; he says (p. 225)

"A cut not less then four inches should be made midway between the last rib and the crest of the ilium. It is of the utmost importance that the deeper incisions be kept the same length as the cut through the skin. I am quite convinced that this is the secret of overcoming the difficulties of the operation. If the colon be fairly exposed as I have directed, there is usually but little difficulty in recognising it, even when it is quite undistended, and picking it up from the bottom of the wound."

We have before us a letter from a *confrère*, whom we assisted, some time since, in the operation for colotomy, the case being

one of cancer of the rectum: in reply to our inquiry as to the termination of the case, he writes:

"The patient lived a few days over six months after the operation. I may mention that, after we got rid of the hardened fæces which were lodged between the opening and the rectum, the patient suffered very little pain and inconvenience, and he recovered his appetite. My impression is, that if I had operated much earlier, and so prevented the irritation of the ulcerated surface by the daily fæcal evacuations, he might have gone on much longer. I should also endeavour, in another case, to so manage the opening in the colon that no fæces could pass into the lower part of the bowel, for if by accident any did do so, we were sure to have acute suffering for two or three days."

Mr. Smith convinced of the advantages of palliative colotomy says (p. 72):

"I shall not hesitate, in the first suitable case which comes under my care, attempting to give the relief which most certainly may be better afforded by Amussat's operation than by any other means."

Mr. Allingham (p. 172), in dealing with the subject of "Ulceration of the Rectum," observes:

"In the most advanced stages of ulceration and stricture, where there are several fistulæ and the whole rectum disorganized, as it frequently is, nothing short of lumbar colotomy offers to the patient any chance of life. My experience is that these are really the cases in which colotomy is especially to be recommended. I have now three patients alive who were operated upon years ago; I saw a woman lately upon whom I performed colotomy in the year 1867, and she continues perfectly well. Three of my cases are published in the 'St. Thomas's Hospital Reports' for 1870. It is unfortunate that it is not often one can persuade these patients to submit to colotomy until they are almost in 'articulo mortis;' they have a natural repugnance to the idea of an opening in their loins for the rest of their lives, and so they postpone the operation, often until it is too late. My own opinion is that the operation may be considered, not merely palliative, but curative; in time, I am sure, from the cases I have watched, the rectum will in a great measure return to a healthy condition. When no fæces pass through it the ulceration will heal and the stricture may be dilated; fistulæ will also close, spontaneously in some cases. If we could assure these patients that, should the rectum again become pervious, and the ulceration heal, we could close the opening in the loin, many, no doubt, would willingly undergo the operation, but I feel that this is just what I cannot honestly guarantee. I have recently attempted to close the lumbar aperture in an hospital patient, and at present it has not succeeded. In this case the patient's rectum has become fairly sound; there is no ulceration and no stricture, and

some time since I slit up a fistulous sinus, and that has quite healed. She has often passed some motion per anum, but usually it all comes through the loin.

We seem, therefore, to be in a fair way to have colotomy established as a common operation. We need not despair that improvements in the treatment of the lumbar opening, when no longer the necessity for that opening exists, will make its obliteration as certain as the cure of vesico-vaginal fistula; for but a comparatively short time has elapsed since a severe vesico-vaginal fistula was looked upon as incurable.

The term polypus of rectum appears to be applied to several distinct affections. Van Buren thinks "polypus of the rectum is an uncommon disease, occurring in the majority of instances in children."

We believe that polypus in children is a congenital condition of the mucous membrane, exactly represented by the pendulous tumour of the skin, ecphyma mollusciforme of Erasmus Wilson.

Mr. Hulke ('Path. Soc. Trans.,' vol. xxii) has described the structure of a polypus, removed by him from a young woman, as consisting of a "central mass of a delicate, finely fibrillated, and nucleated stroma, pervaded by large and numerous blood-vessels, the walls of which were very thin. Externally, the stroma became closer and less vascular, and at its outer surface it bore a distinctly papillated layer." The pathological parallelism above suggested is, therefore, more than probable. "There are glandular polypi which may be regarded," Mr. Hulke says, "as local over-growths and out-growths of the normal elements of the mucous membrane, fibrous polypi and fibro-cellular polypi."

Mr. Allingham admits that fibroid polypus is quite rare, but his assertion that such growths resemble in structure almost precisely a uterine fibroid, we are disposed to question. It is, we assume, with reference to small polypoid thickenings to be met with at the upper angle of old stanting fissure of the anus that Mr. Smith declares (p. 82) that he has "come to the conclusion that perhaps next to hæmorrhoidal disease polypoid excrescences, in some shape or another, are as frequently met with in the rectum as any other morbid condition of this part."

Villous tumour of the rectum, or as Mr. Quain first described the lesion "a peculiar bleeding tumour of the rectum," holds, in relation to polypus and to cancer, a doubtful position. This disease may truly be termed rare. Amongst the 4000 cases tabulated by Mr. Allingham we do not observe a single example of villous tumour noted. With regard to the nature of villous tumour, Mr. Quain willingly acknowledging the great assistance microscopical observation affords in skilful and expe-

rienced hands, does not think it too much to say that the clinical history of a case is indispensable in order to affirm the cancerous nature of any rare morbid growth, and he accordingly inclines to the belief that villous tumour is non-malignant. Mr. Sibley ('Path. Soc. Trans.,' vol. viii), describing a villous growth (from the dura mater of a woman who died of uterine cancer), classes villous along with colloid disease as non-malignant.

If villous tumour of the rectum be really non-malignant it may claim to be classed as a polypus, and thus to constitute a fourth species of polypoid disease.

Dr. van Buren's last lecture contains some valuable remarks in his customary straightforward and impressive style on the means and appliances to be employed in order to form a prompt and accurate diagnosis in affections of the rectum. Any proposed addition to our apparatus for physical exploration is worthy of attentive consideration. Dr. Bodenhamer figures a Recto-colonic Endoscope, it consists of a cylinder fourteen inches long and seven-eighths of an inch in diameter, partly flexible and highly polished, furnished with an internal reflecting mirror mounted upon a rod; but he adds that "this instrument was gotten up with considerable haste, and may upon further trial be found to need some improvements."

It may be observed in the preceding pages we have quoted in several instances, the opinions of writers who have already become standard authorities; we have done this in order to let our readers determine to what degree advance has been made within the past decennium. We think that the frank acknowledgments by both the American and English authors whose names stand at the head of this article, of what they owe to their immediate predecessors cannot but be gratifying to these gentlemen.

III.—Sir J. Simpson on Diseases of Women.[1]

THE third and concluding volume of Simpson's obstetric works is, in many respects, the most valuable part of the recent re-publication, since it contains the record of the author's vast clinical experience, not previously published in a readily acces-

[1] *Clinical Lectures on the Diseases of Women.* By Sir JAMES Y. SIMPSON, Bart., M.D., D.C.L. Edited by ALEXANDER R. SIMPSON, M.D. Edinburgh, 1872.

sible form. The substance of this volume was familiar to all who were privileged to be Simpson's pupils, and parts of his lectures had appeared in the 'Medical Times,' but they had not been collected into a volume, and to the bulk of the profession they will be entirely new.

Gynæcology is year by year advancing with such rapid strides that, short as is the time since these lectures were delivered, they cannot be considered to be quite up to the most recent knowledge on the topics they discuss. Still the volume, as a whole, stands unrivalled as embodying the records of that wonderful knowledge and fertility of resource for which Simpson was so remarkable, and which found their greatest development in the subjects here treated of, since they formed so large a portion of his daily work.

To attempt anything like a critical or exhaustive review of the contents of a volume extending over 700 pages would be far too ambitious a task, and one which could not be attempted in the short space of one paper. We shall, therefore, content ourselves by referring to a few of the most prominent articles, which are specially interesting either on account of their own intrinsic value, or from the discussions and differences of opinion to which they have given rise.

The first essay on the "Diagnosis of the Diseases of Women" is, perhaps, more widely known than others, since it is a reprint from the first collected edition of his works published by Priestley and Storer. It is peculiarly worthy of study by all who wish to master the subject, dwelling forcibly as it does on the importance of cultivating our powers of accurate diagnosis to the utmost, and pointing out the fact, too often not sufficiently remembered by gynæcologists, that there is nothing special or peculiar about diseases of the uterus; that that organ is liable to the same varieties of ailment as other parts of the body, and that it is a fatal mistake to fix our attention too exclusively on any one class of symptoms, or to refer them, as so many are apt to do, to one class of causes. Were these facts sufficiently remembered we should not so often meet with obstetricians who can examine no woman without finding inflammations and engorgements of the womb, and others who discover that every woman has her womb twisted out of its normal shape. Special stress is laid upon the sympathetic derangements which so commonly attend upon uterine disease, and a knowledge of which will guard us from overlooking the true cause of disturbances apparently unconnected with the uterus; untractable to all treatment that does not trace the symptoms to their true source, and readily cured when their real nature has been ascertained. Cases of this kind must be familiar

to every obstetrician, and it is to be regretted that their frequency is by no means recognised by the bulk of the profession.

The first of the clinical lectures treats of vesico-vaginal fistula and its cure, and describes the operative procedures Simpson himself used. He was an ardent admirer of the metallic suture, and attributed much of the modern success of the operation to its use. Inasmuch, however, as the disease has recently been made the subject of a special article in this periodical, in which the question has been fully discussed, we need not refer to it more at length.

The next two lectures treat of pelvi-cellulitis and pelvi-peritonitis. The first of these was written many years ago, and before the existence of the latter form of disease was recognised, and the two classes of cases were evidently confounded together. In making this mistake Simpson certainly did no more than his contemporaries, and the first of his two lectures is an admirable *résumé* of all that was then known of a disease which his own writings did much to familiarise to the profession. A recent reviewer of this work blames the author for modifying his views with advancing knowledge. Few, however, we think, will be inclined to agree with him, and surely it is creditable rather than otherwise, that Simpson should be ready, even so lately as two years before his death, to avail himself of the most recent researches on the subject, and embody them in a lecture clearly showing that they were not adopted carelessly or hurriedly, but only after mature reflection had convinced him of their soundness.

In the first of his two lectures there is not much that obstetricians are not now familiar with; although at the time that it was originally published, its teaching was much more novel. In regard to artificial evacuation of pelvic abscesses after the formation of matter, too early a resort to surgical interference is deprecated; and when absolutely necessary, the use of a guarded tenotomy knife is preferred to the trocar or other means. Considerable exception may no doubt be taken to the practice of passing the finger through the opening thus made, and breaking down forcibly any dissepiments that may be felt. It seems more than questionable whether such rough practice would prove either safe or useful. Of the occasional utility of making a counter opening for the free escape of pus in old standing cases some curious instances are given.

Of the supplementary essay on pelvi-peritonitis we need only say that it embodies in a very clear and readable way the teachings of Bernutz and other modern writers on this important subject. The defects of the lecture seem to us to be, that the

author does not recognise sufficiently the secondary effects of pelvi-peritonitis, in fixing the uterus and leaving adhesions, often of long standing and leading to obscure and troublesome symptoms. This is a point but little dwelt on by any author except Dr. Matthews Duncan, in his work on "Para-metritis and Peri-metritis;" and yet when attention is directed to it cases of the kind will be very frequently detected. These names, adopted by Duncan from Virchow, are strongly objected to by Simpson, who brings much classical criticism to bear on them. To us they certainly have the disadvantage of not indicating in any way the distinction in the nature of the two affections with regard to the tissues mainly involved; and farther, from their resemblance to each other in sound, they seem apt to lead to confusion.

The next lecture discusses the subject of hæmatocele. This was probably written before the appearance of several recent monographs, which have thrown considerable light on various points of the disease, and there are several statements contained in it which are scarcely in accordance with the most recent views on the subject. Thus Simpson seems to consider it beyond question that the extra-peritoneal seat of the effusion is the most common, and that the intra-peritoneal is comparatively rare. We need hardly point out that the reverse of this is now generally believed to represent the true state of the case, and that Bernutz and other authorities are even inclined to question the existence of the extra-peritoneal effusion. Then, again, it seems to us that he much underrates the frequency and importance of the inflammatory complications attending on hæmatic effusions. The practice of freely opening enlarging hæmatocele, and breaking down the contained blood clots, is one certainly attended with risks which are nowhere alluded to, and one also, which at least should have been recommended in a more guarded manner.

The next paper, on "Cancer of the Uterus," is a very fair and exhaustive account of this distressing disease, well worthy of careful study, from the number of suggestions as to treatment it contains. Many of them, such as the injection of vapour of chloroform, of carbonic acid gas, and the like, have been, no doubt, too highly estimated by Simpson. But few will read this paper who will not find that the author's fertile genius has suggested some novel expedient previously unknown to them. We observe that Simpson fully recognises the existence of cancer of the body of the uterus, the cervix remaining unaffected, and the uterus being freely movable. Cases of this kind are very apt to mislead, and we are inclined to suspect that they are somewhat more common than is generally sup-

posed. The advantage of amputation of the cervix in cases of cauliflower excrescence is fully recognised, and some interesting cases of its successful practice are recorded. Next we have a full account of the uses of various caustic applications, chiefly with reference to the result of their employment in mammary cancer. The situation of the uterus, of course, requires a method of treatment peculiarly difficult, and thus far we have not much success to boast of. The current of modern opinion is now much more decidedly in favour of the local origin of malignant disease than at the time Simpson wrote, and its cure, under favorable conditions, seems less hopeless than before. It hardly seems utopian to hope that we may yet find some local application which will at least check the exuberant growth of the cancer texture, and even eradicate it altogether, when we are able to detect it at an early stage.

The next essay, on "Coccygodynia and Deformities of the Coccyx," treats of a subject which was always a favourite one with Simpson, and contains much curious information on lesions which have been comparatively little studied, and which will not be found described in any of our gynæcological text-books. By the term coccygodynia the author designates a peculiar painful affection of the terminal coccygeal bones, accompanied by great suffering on locomotion, or when the bone is pressed upon *in sitû* on lying on the back. Some of these cases are apparently referable to the class of neuralgias, and are best remedied by general anti-neuralgic treatment, combined with appropriate local sedative applications. The majority of them are, however, referred by Simpson to some unexplained local affection of the bone itself; great suffering following when the bone is moved by the muscles attached to its surfaces and borders. This he proposes to cure by the simple operation of dividing the ligamentous and muscular attachments to the bone, by a subcutaneous incision with the tenotomy knife, and in various instances in which he has practised it he has apparently effected very remarkable cures. Thus we are told of one lady who for several years had been a martyr to sufferings in this region of her body, which all the skill of various attendants had been ineffectual in removing, and which disappeared as if by magic on this slight operation being practised, and of a still more curious instance of a lady in whose tissues the tenotomy knife broke during the performance of the operation, but who became immediately reconciled to the presence of this foreign body left in her from the instantaneous removal of the pain from which she had so long suffered. In one or two particularly rebellious cases Simpson has practised the excision of the offending bones themselves with apparent success. For ourselves, we should be inclined to doubt the

great frequency of this affection, although Simpson met with it no less than ten times within three weeks. Certainly, cases of the kind do not seem to be by any means so common in London as they appear to be in Edinburgh; but the subcutaneous division of the muscular attachments of the bone is a simple and, apparently, not a hazardous expedient, well worthy of being kept in mind when cases do happen to come under our notice. The remainder of the essay contains a curious and interesting account of the accidents sometimes arising from injury to the coccyx during parturition, not dwelt on in any of our text-books, and a discussion on the nature and treatment of those curious cases of sacral tumour from monstrosity by enclosure, of which most of our museums contain specimens.

The essay following on "Dysmenorrhœa" will no doubt be unsatisfactory to the modern school of gynæcologists who of late have laid so much stress on the influence of uterine flexions in producing painful menstruation. It seems strange that so practical a physician as Simpson, who was himself the chief introducer, if we may be permitted the expression, of uterine flexions into British gynæcology, should not even allude to them as a cause of dysmenorrhœa, if, as we are taught, they are in reality the chief, or one of the chief, causes of that disease. It can hardly be supposed that if they have the all important bearing on the subject, which we are called on to believe, that they should not have attracted his attention at all in connection with this subject. In saying this we have no wish to be considered as doubting their influence when existing, or even as questioning their very frequent occurrence, but we believe that a large number of cases of dysmenorrhœa have no connection with displacement of the womb, but may rather be referred, as Simpson tells us, to the congestive changes taking place in the ovaries and the uterus in connection with the maturation and rupture of the Graafian follicle. The modern mechanical view has the effect of diverting our attention from the important class of ovarian dysmenorrhœas, of which we have examples occurring in women in whom no uterus was found to be present. The same error of over-estimating the importance of one particular class of obstructive causes is well illustrated in this paper by Simpson himself, who attaches excessive importance, as most obstetricians will now admit, to contraction of the canal of the cervix, and recommends his own special panacea of division of the cervix with a freedom and carelessness which was one of the most unfortunate parts of his teaching. We are all familiar with the stories, so rife in the north, of the disastrous results which are said to have occasionally followed his indiscriminate use of the hysterotome, and it is much to be regretted that there is so little said in these lectures

as to the possible risks, or of the precautions to be taken before resorting to this operation.

The next paper treats of closure and contraction of the vagina, and of the retention of the menstrual fluid which sometimes accompanies them. Simpson fully recognises and points out the danger which occasionally follows the apparently simple operation of giving exit to the retained fluid. It is certainly a curious fact that a procedure apparently so simple as incising the septum which has caused the occlusion, should so frequently have occasioned serious and even fatal symptoms. Nor are we able to explain the fact satisfactorily, or to decide with certainty as to the best means of obviating the danger. Opinions are still very various on this latter point, but Simpson holds, and we think rightly, that it is better to make a comparatively small opening and permit the uterus to expel its contents—guttatim—rather than to make a free incision, and attempt to clear the uterine cavity at once by intra-uterine injections.

Fissures of the orifice of the vagina, similar to those so commonly met with about the anus, are treated of in the same paper. These are not described in any of our gynæcological works, are not very often recognised, and are frequently the cause of much suffering. The author recommends for their cure the simple operation of a linear incision through the mucous membrane, precisely as in the analogous fissures of the anus, and apparently this simple procedure is extremely effective.

The elaborate series of lectures which follow discuss at length the subject of "Surgical Fever," which, from the light it throws on the analagous affections of the puerperal state, was always a study in which Simpson took especial interest. His views on this topic have so frequently been described and criticised both in this journal and elsewhere, that we can afford to pass them over without comment, the more so as they are of greater interest to the surgeon than to the gynæcologist. Our surgical friends receive a not entirely unmerited rebuke for the slight attention they have paid to this subject, which to them should be all important, and it is not altogether creditable to them that the most valuable monograph upon it yet written should have come from the pen of an obstetrician. Although there is, no doubt, much in these lectures which more modern researches would require us to modify, they can never fail to interest from their erudition and suggestiveness, and they will well repay the most careful study.

The next paper treats of phlegmasia dolens, its pathology and treatment. With regard to the cause of this well-known complication of the puerperal state, and its relation to pregnancy,

pathologists are by no means yet decided. Simpson adopts the view so ably maintained and experimentally illustrated by the late Dr. Mackenzie, that it is essentially a blood disease, depending partly on the alteration of the blood accompanying pregnancy, and partly on the additional blood changes produced by the presence of the large amount of effete matters present in the circulation after delivery, to which may probably be added certain septic changes from absorption through the uterine veins.

That local inflammatory changes in the coats of the vessels are not of themselves sufficient to produce the disease appears to us abundantly evident. While we cordially agree in the main with Simpson's views as to the causation of the disease, it seems to us that his essay is defective in not pointing out the close connection between this disease and those appalling complications of pregnancy resulting from clotting of blood in the pulmonary arteries, or the impaction in the same vessels of a travelled embolus. Both these conditions, which should be carefully distinguished from each other, are doubtless the result, as recent researches have clearly shown, of precisely the same blood changes as those which produce the characteristic appearances of phlegmasia dolens. In the former we probably have the precise analogue of the local blood-clotting which precedes the œdematous swelling in phlegmasia dolens; the only difference being that, in the one case, the thrombosis primarily occurs in the pulmonary arteries, and from its situation at once blocks the circulation at a vital part, and causes more or less instantaneous death; while, in the other, the thrombosis occurs in distal vessels, and leads to the well-known secondary consequences so often observed. In the latter we probably have an entirely secondary result of a primary thrombosis, and this form of pulmonary obstruction can only occur after the clot originally formed in the distal vessels has had time to soften, so as to admit of a portion being detached and carried along the current of the circulation, to be impacted in the pulmonary arteries. Such being the case, it appears to us that any account of phlegmasia dolens which does not embody a description of these formidable diseases is necessarily defective, and it is to be regretted that Simpson has followed the usual custom of treating them as entirely distinct and separate affections. One recommendation as to treatment given by Simpson, as well as by most writers on the disease, seems to us highly objectionable. We refer to the use of repeated friction of the affected limb. The objection to this of course is, that it might possibly aid the separation of a portion of the coagulum in the veins of the diseased extremity, and so lead to death by embolism. We are not aware that friction

ever has been known to produce this result, but that it is capable of doing so is undeniable.

The essay on spurious pregnancy, or pseudocyesis, as it is somewhat pedantically named, is extremely interesting, and is the best monograph existing on this strange affection. It abounds with quaint anecdotes, in the collection of which Simpson was such an adept, and were it not that most obstetricians could match them from their own experience, they would be apparently beyond belief. With regard to the pathology of the disease, Simpson does not give us any very satisfactory information. He shows us that it is not necessarily connected with any marked uterine or ovarian disease, although often associated with it, and believes that it is produced by undue ovarian irritation, connected with the function of menstruation, and to this being repeated from month to month, before the sympathetic phenomena of the previous period have had time to subside. This does not, however, seem to us to throw much light on the subject. With regard to diagnosis, Simpson's suggestion of the use of chloroform affords the simplest and best means of removing all doubt, and we have found it peculiarly serviceable not only as assisting our own diagnosis, but as a ready means of convincing the patient's friends of the phantom nature of the swelling.

The succeeding lectures on ovarian dropsy occupy no less than 116 pages of the work. Written many years ago, and long before ovariotomy had become the well established and successful operation it now is, these lectures of necessity are not to be taken as embodying our most recent knowledge on this most important disease. In spite of this drawback, they form an excellent monograph on the subject well worthy of the most careful study. What is said of the history of the disease, and the differential diagnosis between ovarian tumours and other forms of abdominal growths, is probably as good a compendium of our knowledge as has yet been written.

The peculiar value of these lectures, however, consists in the fact of their conclusively showing that Simpson had fully recognised the value of the operation, and had thrown the whole weight of his influence into the scale in its favour, at a time when to do so implied some boldness, and placed the author in a position of antagonism to the bulk of the profession. Now that every surgeon approves, and most surgeons perform, ovariotomy, the merit of this early recognition of it is apt to be overlooked, yet it is certain that Simpson's teaching and example have had considerable influence in rendering the operation popular. He very clearly shows that, even at that time, the operation was not more dangerous than many others performed daily, and he dis-

tinctly enunciates the opinion, which subsequent experience has so clearly corroborated, that with increased care and improved methods of operation the mortality would come to be very materially lessened.

The next essay is more purely obstetrical than any of the preceding, and discusses the various operations involving the destruction of the fœtus. It commences with a powerful and eloquent protest against the resort to craniotomy where possibly forceps or turning may have rendered that dreadful alternative unnecessary.

This protest was doubtless more necessary at the time it was written than it is now; but, even yet, it is to be feared that there are practitioners who regard the deadly perforator with less horror than it merits. It is unquestionable that the facility of the operation has often led to its performance when other means might have saved the life of the child without materially increasing the risk to the mother. It is lamentable to think what harm the erroneous teaching of British obstetricians, especially those of the Dublin school, may have done in this way; and when we reflect that even when the frequency of craniotomy was much diminished in the Rotunda Hospital, under the masterships of Drs. Clarke and Collins, craniotomy was performed three times as often as forceps delivery, we cannot but look with horror upon so unnecessary a destruction of life. The observations of Simpson on the responsibility of the practitioner may be worth quoting, and will doubtless meet with general assent:

"There may be cases of such a kind that recourse to this operation affords us the only means of escape from a painful and perplexing dilemma. But I greatly fear that recourse has been had to it in these islands far, far more frequently than the exigencies of practice have warranted. Mark the contrast: how carefully our laws guard against the possible unnecessary destruction of human life under all circumstances, and yet how carelessly our obstetric laws guard against the destruction of the life of the fœtus during birth. If a man is evidently guilty of the most dreadful crimes, yet before he is condemned and executed, witnesses are allowed to be called in his defence; barristers are allowed to plead for him; juries and judges sit for hours or days hearing and balancing all the evidence for and against him. Yet the child, immediately before birth, has often been sacrificed and destroyed by craniotomy, with little or no examination as to the absolute necessity of the act; the attendant practitioner acting in his multiple capacity of witness, barrister, jury, judge, ay, and of executioner too. Murder, *ante partum*, is thus often performed by one man, on his own undivided responsibility, while, I fear, the infliction of any form of injury on the fœtus is altogether unwarrantable."

The more immediate object of his essay is to recommend the

breaking down of the base of the fœtal skull, where craniotomy is essential, rather than its removal piecemeal, and the arguments in its favour are forcibly put. The plan advised for this purpose is the use of his cranioclast, which effected doubtless a great improvement on the usual methods of operating. The more easy and more effective operation of cephalotripsy is now so universally approved of in this country, although we can hardly say as yet universally adopted, that it renders any criticism of Simpson's operation unnecessary. Curiously enough, Simpson had at the time tried cephalotripsy with a result that induced him to write, "I hardly think it likely that I shall ever be induced again to try it." This failure may doubtless be attributed to the use of an imperfect instrument, since, as is well known, he afterwards became an ardent advocate of the operation, and invented one of the best patterns of the cephalotribe we possess.

The following article treats of an obscure and comparatively little studied subject, viz. dropsy and other diseases of the Fallopian tube. The most important of these is undoubtedly the so-called Fallopian-tube dropsy, the etiology of which Simpson, no doubt, correctly attributes to occlusion of either extremity of the tube, with inflammation of the lining membrane, and distension of the calibre of the tube from the subsequent accumulation of the canal of the fluid poured out. So far most gynæcologists are of accord, but we fancy that Simpson has unwittingly exaggerated the facility with which such tumours may be detected and diagnosed. The diagnostic marks he lays down are—(1) its free and independent mobility, (2) its elongated form, (3) its wavy outline. None of these, however, would seem to be very reliable tests, for the tumour may be, and very often is, fixed from surrounding pelvic adhesions, so that its mobility is lost, while the detection of its wavy outline and elongated form involve an accuracy of diagnostic power which will not often be brought to bear on them. The only available treatment is, however, that recommended by him, viz. tapping with a hair-like trocar, so as to admit of the escape of the fluid, and fortunately this is so harmless a proceeding that it is not likely to give rise to any trouble, even when a mistake in diagnosis has been made.

The next paper which follows is on puerperal mania, a topic of greater interest and practical importance. Its main object seems to be to prove the existence of a close connection between the disease and a toxic condition of the blood, indicated by the presence of albumen in the urine. This he discovered in eight out of ten cases in which he searched for it, and the difference between these results and those of others, who have not noticed the condition, is attributed to the fact of the

albumen generally disappearing from the urine within a very short time of the commencement of the disease.

What the precise relation between albuminuria and puerperal insanity is Simpson does not explain, leaving it to be discovered in that happy time when pathological chemistry has reached its climax of success. We are not aware that the subject has been particularly investigated since the publication of this essay, still the albuminuria has not been admitted to have anything like the importance here attributed to it. We have ourselves repeatedly failed to detect albumen in the urine of patients suffering from mania, although the search for it was made very shortly after the outbreak of the disease. That albumen is common enough in the debilitated patients who are the most frequent sufferers from the disease is unquestionably probable, but that it has the all-important influence attributed to it is more than doubtful, and, at any rate, it is a theory by no means proved.

The next paper treats of sub-involution of the uterus, that peculiar state of hypertrophy which so frequently results from some interference with the change which should reduce the enlarged uterus of pregnancy to its natural size. This condition is unquestionably one of great frequency, and enables us to explain many very obscure and obstinate forms of uterine disease. We look upon its recognition as one of the most important of Simpson's contributions to gynæcology, and one, perhaps, not even yet estimated at its proper value. Certainly, any one who has once had his attention directed to it will not fail to discover it with a frequency which Simpson has not overestimated, and in its detection the uterine sound, used as recommended in this paper, is of immense value. What we are here taught of the causes, natural history, and symptomatology of this disease, leaves little to be desired. With regard to the treatment, perhaps the same cannot be said. Simpson seems inclined to recommend chiefly local antiphlogistic measures, such as leeching, with the administration of internal deobstruents, such as the iodide and bromide of potassium, and the persevering use of counter-irritants over the pubes. That relief may be obtained from this treatment is unquestionable, but in our experience far more speedy and satisfactory results follow the direct application of suitable local treatment to the enlarged uterus itself, especially of intra-uterine applications in well selected cases, and the use of pledgets of iodized cotton to the cervix through the speculum. The introduction of the latter means of treatment we owe, we believe, to Dr. Greenhalgh, and it is surprising how effectual it is in reducing rapidly to its proper size the large, tender, and hypertrophied womb characteristic of this form of disease. It has moreover, the advantage of being much less irksome and

painful to the patient than the continuous counter-irritation Simpson recommends.

The allied condition of super-involution of the uterus, which is comparatively rare and of secondary importance, is made the text for a very full and able essay on the subject of amenorrhœa, which is well worthy of study, but which we have no space to enter into. It is of interest, however, as containing a recommendation of the intra-uterine galvanic stem pessary in cases in which the absence of menstruation can be traced to the presence of an under-sized and imperfectly developed uterus. There are few of Simpson's peculiar methods of treatment which have been so freely criticised or more opposed. There can be no doubt that it is a plan capable of doing much injury from the excessive local irritation it may produce, and that it is recommended without sufficient warning of its possible evil effects. It is equally certain that in properly selected cases it may prove of much value. The true explanation of the very contradictory opinions held with regard to its failures is that it has often been employed when the uterus is in a state of chronic irritation and congestion, especially in cases of retroflexion, when it will almost certainly do harm. In cases of amenorrhœa, such as are described in this paper, dangerous results will not be so apt to follow its use, and to such, it seems to us, the use of the intra-uterine stem should be exclusively limited. Even then, however, its effects should be watched with a degree of care and even anxiety, the necessity of which Simpson apparently failed to see.

Passing over several papers of subordinate interest we come to a series of lectures on fibroid tumours of the uterus in which a very complete account is given of this common form of uterine disease, of the changes which these tumours may undergo, and of the best means of treatment, both medical and surgical. It is interesting to note that so accurate an observer as Simpson recognised the fact, recently stoutly denied, that nature occasionally does for us what art is unable to accomplish, and completely removes these tumours by a process of spontaneous absorption. This fact is no doubt a strange one and difficult to believe, but it is supported by such a mass of evidence that it is not easy to understand how the occurrence is yet received with so much scepticism. It is satisfactory to those whose opinions on this point have been received with ridicule to find them supported by so high an authority. The explanation which Simpson gives of the phenomena seems unquestionably to be the only way of accounting for this obstetric puzzle. He says—

"When we remember that they are not heterologous growths, but

a mere nodose hypertrophy from the middle coats of the uterus, and when we see the change of growth and involution that occur in the fibres of that middle coat during pregnancy, and after parturition, we may surely hope that the adventitious fibroid masses may become reduced in the same way by a sudden process of fatty degeneration."

As an illustration of what Simpson actually observed we may quote the following cases—

"There are two ladies from the north of England whom I have seen from time to time; one from Alnwick during the last five years, the other for three years, both of whom are the subjects of fibroid tumours of the uterus, and both of whom believed, the last time I saw them, that they were completely cured. In both of these the tumours, when I first saw them, reached nearly to the umbilicus, and now they are so far reduced in size as to have passed beyond the patient's ken."

The most sceptical will hardly suppose that Simpson could have made an error of diagnosis in cases of this kind. It is to be deplored that we are, as yet, quite ignorant of any means of originating this change; but, surely, it is not too much to hope that eventually a careful study of nature's occasional cure may enable us to hit upon some method of promoting or favouring it. The account given of his surgical treatment of fibroid tumours can scarcely be considered as embodying our most recent knowledge on the subject, and the papers of Matthews Duncan and others who have written on the subject, teach us that much more can be done in controlling hæmorrhage than is generally believed to be the possible. Indications, too, are not wanting that gastrotomy for the removal of large uterine tumours, imperilling the life of the patient, may yet come to be considered a more hopeful and justifiable operation than it is now thought to be. Those who wish to learn what success may attend such apparently desperate measures will find some valuable cures narrated by M. Péan, of Paris, reported in the last number of the Review.

The remaining papers treat of the various forms of metritis and their treatment, and the author recognises the necessity of suitable intra-uterine applications, although he evidently had not paid much attention to this most valuable method of treatment, and used very imperfect means of applying it. Finally, we have some lectures on flexion of the uterus, a favourite subject with Simpson. It is to be feared that the extreme, and, as we believe, in the main, mistaken opinions he held on this topic have done much to foster the exaggerated estimate which many modern gynæcologists take of the importance of these

affections. We have no space, however, to do more than allude to the subject, nor is it necessary, since we have fully discussed it in a recent number of this Review.

That this volume will explain to posterity the position which Simpson had gained for himself cannot be questioned, and in studying it it will be easy for the obstetrician of the future to see why its author had acquired so great and well-earned a reputation.

IV.—The Pathology of Hysteria.[1]

In a paper read at the annual meeting of 1871 of the British Medical Association, by Dr. Tilt, the point consisted in drawing the attention of the profession to the sharp division into two camps of those who have interested themselves in the pathology of Hysteria. To the one the female generative organs are the whole and sole seat and origin of the disease; to the other its essential nature is quite independent of these parts, and its existence is no evidence of their being abnormal. In the first are ranged those who have devoted their lives to midwifery and the diseases peculiar to women; in the second a number, a smaller number by Dr. Tilt's reckoning, whose business it is to teach general pathology and therapeutics. As far as this country is concerned, the sketch of the state of parties at that date seems accurate; and, indeed, one can scarce read any publication on the subject without being offended by the evident tone of advocacy, and the frequent use of the words "triumph," "contend," "confess," "assert," "allow," "pretend:" as if truth were worth less than victory. No one can help joining with Dr. Tilt in thinking this discreditable to the

[1] 1. *On Hysteria and its Interpreters.* ('British Medical Journal,' Dec. 16th, 1871.) By E. J. Tilt.

2. *Article "Hysteria."* ('System of Medicine,' vol. II, London, 1868.) By J. R. Reynolds.

3. *Clinical Lectures.* Fourth Edition, 1865. (Lecture XXXII.) By T. King Chambers.

4. *Studies on Functional Nervous Disorders.* By C. Handfield Jones. Second Edition, London, 1872.

5. *Pathology, Diagnosis, and Treatment of the Diseases of Women.* By W. Graily Hewitt. Third Edition, 1871.

6. *Etudes cliniques sur l'hystérie.* By E. Chairou. Paris, 1870.

7. *Traité complet de l'hystérie.* By H. Landouzy. Paris, 1846.

8. *Traité clinique et thérapeutique de l'hystérie.* By P. Briquet. Paris, 1859.

9. *De l'état nerveux ou Nervosisme.* By Bouchut. Paris, 1860.

10. *On the Pathology and Treatment of Hysteria,* By R. B. Carter. London, 1853.

British school of medicine; for the absoluteness of the platforms on an obscure scientific question is alone enough to convict both of error.

Dr. Tilt attributes the sectarianism of the British profession to the fact of the feminine sexual ailments being made a speciality much more than is usual in other lands. In France, as he observes, either Trousseau, or Chomel, or Nélaton, would in his hospital ward examine and take charge of a case obviously and professedly uterine; whilst in London a Jenner or a Jones or a Salter would not even determine whether it were really uterine or not, but would hand it over to be examined by the Accoucheur. This is quite true, and is with some justice accused of leading to the narrowness of view detected by Dr. Tilt.

It is not, however, the whole of the cause, for there may be traced even in French authors, under a de-specialised dispensation of hospital schools, still an undercurrent of party spirit on the subject, most unbecoming to scientific physicians. It appears to a calm looker-on that a vast deal of the disagreement about the pathology of Hysteria, not only as between the opposing hosts, but between many of the men who compose them, is due to hardly any two of the combatants using the name assigned to the disease in the same sense. This is shown by the inevitable length of the chapter on diagnosis in all monographs, especially French. And it may be observed that whenever any of these writers finds his arguments in peril of being shaken by the facts of some of his opponents' cases, he invariably says he does not consider these as examples of Hysteria at all—they are not what he calls Hysteria.

Probably the broadest use of the appellation is made by Sydenham, who in a rough estimate says that it is applicable to at least one in six of medical cases, that is to say, to one half of the chronic cases among females. But that large proportion is accounted for when in the next paragraph we read that he declines to distinguish between Hysteria and Hypochondriasis, which he pronounces as like as two eggs—"*vix ovum ovo similius quam sunt utrobique phænomena.*"[1] And when shortly afterwards he admits us behind the scenes in his study, we learn that whenever his advice was sought by a woman with symptoms due to a cause not immediately detected by the common methods of investigation, he asked first if she had suffered any sorrow, anxiety, or other perturbation of mind, and then if she passed at times an excess of watery urine. An affirmative settled the diagnosis for Hysteria.

On the other hand, the narrowest limits,—at least we cannot

[1] 'Epistola ad G. Cole, M.D.,' 59. Dr. Swan is shocked at the colloquial expression, and dilutes it into "we find a great similitude between them."

conceive narrower, placed to the employment of the word, is due to the military precision of an army surgeon, M. Chairou, who defines it "a neurosis, which takes its origin in congestion of the left or both ovaries, and determines a paralysis of the epiglottis, first followed by other nerve disorders." He would not allow cases similar externally to be called by the same name, and says that the nerve-symptoms excited in cats by valerian cannot bear any relation to Hysteria, because the tom suffers as much as the puss. If men are ill in the same fashion, their convulsive sobbings are "hysteriform;" and even a woman may have an hysterical fit without being classed by M. Chairou as hysterical, unless she have congestion of the left ovary and anæsthesia of the pharynx:—"*La crise hystérique, ou convulsive, ne constitue pas l'hystérie—la crise hysteriforme peut exister sans l'affection même.*"[1] Did everybody's trumpet give as certain a sound as this, it would be clear enough what each author meant by his title. But that is far from being the case, and we are left to work out by the inference of internal evidence what ailments are to be included, and what not, under the common heading. Thus we find in 'Reynolds' System of Medicine' separate essays on Exstasy, Catalepsy, Somnambulism, and Hypochondriasis; so that Dr. Reynolds evidently, when he writes afterwards of Hysteria, does not view the aforenamed as part of it. On the other hand, the College of Physicians, by omitting all mention elsewhere of Exstasy and Somnambulism, implicitly includes such states as Hysteria. Dr. Briquet, in a work which heads this article, again specifies Catalepsy as a symptom of Hysteria, but designates Hypochondriasis as its direct antipode. He thinks it so different that it cannot be mistaken. On the other hand, Sydenham, as we have seen, makes little distinction between them; Dr. F. Dubois (a well-known alienist) thinks them at any rate so alike that he writes a stout octavo volume in order to draw the line. Far be it from us to blame; the substantive is common property, for each one to define as he likes. The only really annoying sinners are those who use it in a narrow sense in one part, and in a broad sense in other parts of their lucubrations, without giving notice, and often within the limits of a short paper or letter.

Some pathologists are distressed at the established nomenclature, thinking that a connexion with the Greek, which lies on the surface of the name as written, unfairly prejudges the question. Such elaborate terms as *encéphalie spasmodique* (Georget) and *neuropathie aigue cérébro-pneumogastrique* (Girard) have been proposed. But these ponderous substitutes

[1] 'L'Hystérie,' p. 11.

really prejudge the question still more glaringly, and, moreover, do not distinctly characterise the malady. With all due respect to name-fanciers, the best *nomina propria* are those which mean the least. We must plead guilty to a sneaking attraction towards a singularly senseless synonym printed in M. Landouzy's list, "*l'amarry*" (*Traité de l'hystérie*, p. 14), only we are suspicious of it being a misprint, like "*Splenn*," a few lines further on. But there is no cause for alarm; medical men are little led by Greek derivations; and if they were, Northerners have done their best by pronouncing the second syllable long, to dissever the connection of Hysteria with the genital organs. Hyste*e*ria in Greek ("a feast at which swine were immolated," 'Donnegan's Lexicon'), can call up nowadays no idea but that of a pig-sticking tiffin, or a Polynesian revel.

The remedy for the confusion lies in one word, "Definition." Let each author commence with a short differentiated description of what he means by the disease he writes about. We regret bitterly that the College of Physicians, in preparing their Nomenclature of Diseases for Great Britain, let slip a golden opportunity of doubling the value of their publication by defining all non-technical phrases and all maladies which are the same in the vulgar as in the scientific tongue. There are just many enough definitions given to show that a committee existed to whom the work was allotted, and few enough to show that it was either too large or too timid to do it effectively. A hope may be expressed that in the next edition, due May, 1879, blanks will be filled up. Meanwhile, caution must be exercised in accepting conclusions from statistics on such a matter as that now before the reader, where hardly any two authorities are agreed as to the exact limits of the term.

It is fair that an example should be set without delay, by the declaration that in the following pages "Hysteria" will mean *a disease of the nervous system, whose morbid phenomena consist mainly in perversions of the vital acts which manifest affections and passions* (Briquet), and "hysteric fits" will mean *acute paroxysms of the same phenomena.*

This definition does not embrace either Catalepsy, or the various forms of functional Neuralgia, or Hypochondriasis; which the College, by naming them separately, distinguishes from Hysteria, though it points out the alliance by their juxtaposition. At the same time, Exstasy, Tarantism, Tigretier, Nymphomania, Satyriasis, Somnambulism, and allied states, which do not appear in the College list, will come under such a definition as here adopted; and hysterical Hyperæsthesia and Anæsthesia will be, as expressed by a star in the Nomenclature,

secondary affections of which Hysteria is the disease. So we are doing our best to conform to lawful authority; but of course we are at present in the position of an officer of the watch reporting noon to the captain; till he says "make it so," the time of day is an open question; and it may be not superfluous to justify and explain the definition a little further.

Throughout the animal creation there may be observed a class of voluntary, or semi-voluntary, muscular acts, which are innately associated with emotions.[1] They are shown to be innate, by being similar in all nations, by being the same *mutatis mutandis* in beasts and men, and by being useless for any ulterior purpose when exhibited by the higher animals. The cooings and cuddlings of affection, the snarls and frowns of hate, the struttings of pride, the crouchings of submission, and a variety of motions of the loins and buttocks, shrugs of the shoulders, tearings of the hair and clothes, &c., are equally intelligible to the Polynesian, to the Esquimaux, and to their domestic pets. Nobody has explained why wrigglings of the pelvis and jerks of the legs should denote joy; but when we see a raven dancing round a dying cow, or at the window of one of our patients, we read his thoughts, and we recognise a common language quite independent of what we are pleased to call our higher reason.[2] Neither the savage nor the cat can hope to destroy an enemy by spitting, but they thereby prove their kinship to those whose saliva is a valuable weapon of offence.

> "The fox, who ne'er so tame, so cherished and locked up,
> Will have a wild trick of his ancestors,"

finds an antitype in M. de Chaillu, scion of a race whose mission is to civilise mankind, expectorating in the face of an opponent at the Geographical Society. That these acts were primitively excito-motory is possible, but they are not so in the superior animals, still less in man; though some of them, such as laughing, shrieking, &c., are exhibited by parts whose usual stimulation is reckoned as reflex. Now it is in the domain of the machinery of these emotional movements that Hysteria is found; every one of its muscular contractions may

[1] Emotions = "vivid feelings, arising immediately from the consideration of objects perceived, or remembered, or imagined, or from other prior emotions." (Brown.)

[2] Of some modes of expressing emotion singular records are preserved in ancient languages. The "sounding of the bowels" strikes readers of Holy Writ as a sign of affection most unlikely to induce a return, and utterly inexplicable; but any one who has been at the pains to educate a young cock parrot will have found that if the bird be allowed to lick and fondle the master's hand, he will (till taught manners) often show his love by at the same time noisily expelling the contents of the cloaca.

be described as either an exaggeration, or abortion, or perversion of an emotional phenomenon; and when glandular secretion is affected, it happens usually in those parts which are much at the mercy of passion, such, for example, as the stomach or the kidneys.

It is very striking to observe how all the strangest vagaries of hysterical paroxysms are reversions to bestial modes of expressing the feelings; barking, mewing, caterwauling, grinning, biting, setting up the back, dashing on the ground, tearing the hair, gulping, and shrieking, and howling, healthy in the lower animals, reappear in us only when we are lowered by disease.

Let it not be supposed, nevertheless, that the feelings, whose normal demonstrations are thus exhibited in caricature, precede or accompany necessarily such an exhibition. The laugh is joyless, the sobs are without sorrow, the tearing of the clothes is not remorse, nor do strainings to the bosom or hand-pressings prove affection. The words and deeds often express the very reverse of the thoughts. "Tears came when I should smile, and smiles when I should blush," says the hysterical Medea of the modern stage. Hysteria is properly placed by the College among diseases of the nerves, not of the mind.

The definition we have proposed is available for either of the sections into which Dr. Tilt complains that the profession is divided. The pathological question on which they would join issue lies deeper. On one side, he says, there are a host of obstetricians, whose experience has led them to consider Hysteria as due solely to the female reproductive organs; and on the other, he quotes three existing London lecturers on medicine—Dr. King Chambers, Dr. Reynolds, and Dr. Handfield Jones—as types of a modern pathological teaching, which denies *in toto* the exclusive proposition of the accoucheurs. Each observer is, in fact, detailing his own experience. An hysterical person who has any uterine discomfort or derangement goes in private, or in public is relegated by the house-surgeon, to an obstetrical physician; whereas persons who have not such ailment, especially if they chance to be of the male sex, come under the care of a non-specialist. This is not justifying, it is only accounting for, the divergences of observation. One cannot excuse Dr. Chambers when he says, at the opening of his lectures, that Hysteria "has no more to do with the organ of reproduction than it has with any other part of the female body, and that it is no truer to say women are hysterical because they have wombs, than that men are gouty because they have beards."[1] He is evidently

[1] Chambers' 'Clinical Lectures,' p. 381.

irritated into an exaggeration. Nor do we think it right for Dr. Reynolds to assume from his own experience, which, as has been shown above, must be limited to a class of cases to the exclusion of others, that "it is the exception, and not the rule, to find any definite malady, or, indeed, definite complaint, in that direction" (*i. e.* of the uterus);[1] and Dr. Handfield Jones[2] looks at Hysteria too exclusively from a psychical point of view. It is wrong to treat the matter in the spirit of an advocate, and because there have been exaggerations in one direction to exaggerate in another, leaving the truthseeker to pick his path as best he can.

Dr. Tilt's cure for the opposition of parties is to abolish specialism in hospitals, with an ultimate view, one presumes, of its abolition in private practice. Midwifery, of course, must be kept as a separate pursuit; but he would have uterine disease treated by the ordinary consulting physician, in order that he may learn and be able to decide finally the place which the genital organs have in the etiology of Hysteria. To the adoption of this remedy must be objected, in the first place, its impracticability, and in the second a doubt of its success in attaining the desired end. Patients, both poor and rich, have made up their minds that specialists for special feminine complaints they will have, when they can get them; and no legislative interference will prevent the supply from following the demand. Hospital authorities will always very properly insist upon their clients having the same supposed advantages as the rest of the population, and would oppose any change of the present practice. What is more, Dr. Tilt's plan would probably fail of its scientific aim. The English profession has by its specialism bred up a great school of obstetrical and hysterological science; and it would surely not promote progress and breadth of view to discourage this. A pupil would not acquire such clear views on the subject, nor acquire them so rapidly, by seeing occasional specimens of uterine disease scattered in the general wards, as by seeing them sorted out and classified as now. The narrowness of party-spirit cannot depend wholly on the specialism, or it would be displayed still more with reference to the teeth, eyes, and ears, than to the womb. The evils of specialism—and it has evils—bear rather on the individual patient than on science, and may be mitigated in other ways than by abolition. Don't shoot your one-eyed horse, you may get a blind one.

With a further intention of promoting sound knowledge, Dr. Tilt would make it a rule that, whenever a woman is hysterical, an accurate examination should be made of the sexual organs.

[1] 'Reynolds' System of Medicine,' vol. i, p. 318.
[2] On Functional Nervous Disorders.

He says, "examined if they present signs of disease." We thought, at first, the printer must have dropped a comma after "examined;" but grammar seems to belie that idea, and the context also; for the object is the instruction of the ignorant teachers and pupils, which would not be attained unless a complete investigation of all took place. But such a proceeding is quite impossible; hysterical women, however weak their will, have a pretty strong "won't," resist stoutly being made martyrs to science, and consider every examination of the body which seems to them needless as an insult, or at best as an indulgence of scientific curiosity. Besides this, in the hysterically disposed an examination is very apt to aggravate the evil it seeks for, and thus the statistical conclusions aimed at would be vitiated.

We anticipate more advantage from the advance of knowledge and the accuracy of observation which specialism tends to promote in the end. Students of nature, in however narrow a sphere, become more and more appreciative of the observations of others in another sphere, and more apt at making just allowances for the varieties of position whence the view is taken. As if to encourage us in our optimism, a new edition has just come out of Dr. Graily Hewitt's volume, in part of which the author gives the result of a nine years' reconsideration of the subject of Hysteria. Dr. Hewitt is in the strictest sense an obstetrician, an accoucheur of the accoucheurs, and the father of them, a school professor of the science, and the president of professors. Yet he is found frankly and loyally expressing his entire agreement with Dr. Reynolds:—"Hysteria is not necessarily associated with disease or derangement of the generative organs of either sex. Such association may and does very commonly exist, but the true nature of the malady may be overlooked, if regard be paid to that particular relation." He adopts these words in spite of the cases of Hysteria which had come under his own care being sufficient to justify a much narrower view. There cannot be too many observers possessed with this spirit of believing others to have powers of observation as well as themselves, and we recommend it for imitation.

In Dr. Hewitt's own clientèle it would seem that wherever Hysteria in the female has been associated with disease of the generative organs, that disease has been such as to induce a chronic flexion of the uterus. On this fact he grounds what he calls a "Mechanical System" of uterine pathology, to account for at the same time these cases and also those, allied to them, usually known as "Irritable uterus" or "Uterine neuralgia." He considers that normally the uterus is retained in a stiff upright posture by a brisk circulation of blood, which maintains

its elasticity and firmness. If from any cause the vessels are atrophied, or even temporarily obstructed, the organ becomes flaccid and folds upon itself. This folding acts like a ligature round the uterus at about its middle, preventing the return of the already partially stagnated blood, and also interfering materially with the circulation in the cervical parts of the organ. It is true that the uterine arteries are outside the body of the womb, and it may be conceded that the bending of it may leave the main trunks as free as ever; but the moment the vessels enter the tissues of the part fed by them they inevitably fall under the influence of compression. Thus ensues a disturbance, which the small anastomotic branch usually connecting the spermatic and uterine arteries cannot adequately compensate. The frequent congenital abnormalities may partially account for their frequent derangement, and the irregular degree in which it affects the individual.

It may be observed, that should this opinion of Dr. Hewitt's as to the frequency of mechanical distortion of the womb as a cause of Hysteria be justified by future experience, it will go far, probably, to explain its hereditary transmission, for what is so likely to become characteristic of a family, a tribe, a nation, as a peculiar form of any organ?

The word proposed by Dr. Hewitt to designate this anatomical condition, "Strangulation" of the uterus, is an unfortunate one; for, from the time of Pliny, that has been the Latin term for hysterical stifling, being intended as the correlative of πνῖξ, which can hardly refer to anything but the glottis. When one wishes to be clearly understood, there is no tongue equal to our glorious English; so if "wrung" sounds too antique, let us say "twisted" or "folded," as descriptive of a womb in the state meant; and if we want to speak of it to an ancient Roman let us call it "*distortio uteri.*" To show that the danger of prejudging the question in relation to Hysteria is not an imaginary one, it may be noticed that Dr. Hewitt is very nearly hanging himself in his own rope; for at page 413, after telling his readers that he had been informed of the original use of the word after his invention of it, he says, "The ancient appellation, therefore, really fits in very well," &c. We cannot imagine a worse fit, if precision of language is aimed at; if not, we must remind our author that throwing mud into a stream is not the way to clear it.

It is right to say Dr. Hewitt adds, in the next paragraph, "Flexions exist without giving rise to hysteria. Hysteria may occur in cases where there is no flexion." So that he would evidently have it understood that he is describing only one of many sources of the ailment.

The fold of the womb, found by Dr. Hewitt in his patients, seems to have been always backwards or forwards. He does not make any allusion to the lateral displacement spoken of by Galen as often associated with hysterical paroxysms.[1] Galen's description is derived from the reports of the physicianesses (*ἰητρίναι*) with whom he consulted, and who may fairly be suspected of imperfect anatomical knowledge. Dr. Hewitt's rests on personal experience, and is probably the more valuable in a clinical point of view. When, however, Galen gets on his own ground, mechanical anatomy, he makes a suggestion which may not be without its value even to the modern professor. He points out the vascular connection there is between the uterus and the contents of the ligaments, the ovaries, &c., and how the swelling of one would compress the other. It may be suggested as a fair matter for inquiry whether, in cases of flexion coinciding with Hysteria, some of the neighbouring organs (say the ovaries or Fallopian tubes) were not mechanically interfered with, and when flexion existed without Hysteria, they were free.

Such would certainly be the opinion of M. Chairou, the title of whose work stands next on our list. In his view the hysterical diathesis, the predisposition to Hysteria, consists in the congestion of the left or both ovaries, or in an inflammation of the broad ligaments; and every chronic disease which causes Hysteria, does so by deranging the menstrual functions, and so, presumably, acting on the ovary. The latter part of the clause is in danger of being invalidated by one of his cases, where Hysteria first appeared before puberty, at thirteen, and the injury to the left ovary is traceable to a premature confinement from a fright, at about sixteen (p. 87). But certainly the greater number seem to show the importance of the ovaries in the chain of causation, independent of disturbance of the womb. This is supported by what may be called an artificial experiment. Dr. Chairou is acquainted with a young person in whom intentional compression of the left ovary will produce an hysterical fit; and Dr. Tilt mentions another similar instance, where pressure on the left ovary is followed by unconsciousness.[2]

In 19 of the 375 cases recorded by Professor Landouzy, the lesion observed to precede Hysteria, or at least to exist with it, was of the uterus itself. Sometimes it was an acute inflammation, sometimes an ulcer, sometimes a prolapsus, sometimes the mechanical disturbance of a pessary, sometimes the result

[1] Galeni de Loc. Aff: L. vi. cap. v.

[2] We may observe that pressure on the ovaria is made by firmly pushing the fingers against the abdominal wall above the fold of the groin. (*Schutzenberger in Briquet de l'Hysterie*, p. 593.)

of astringent lotions, sometimes a tumour, &c. A note of prime interest is, that when the lesion came to a high pitch of destructibility the Hysteria ceased. This would appear to hint that not only a certain degree of lesion, but a certain degree of health, was necessary to the production of the peculiar symptoms; and it would appear to remove to some organ outside the part affected, though physiologically connected with it, the origin of the disease in question. Such an organ is an ovary, and we think, therefore, that Professor Landouzy's cases rather confirm than weaken M. Chairou's view.

The valuable collection of cases from various authors made by M. Landouzy is sometimes quoted as decisive of the opinion of the determining cause of Hysteria being genital in all but an extreme minority of instances. But in point of fact they are not a fair basis for numerical calculation, for, without special reason for the record of ordinary and mild cases, it is obvious that reporters will select for publication those only which are strange, or which present strongly marked features of anatomical lesions, so that their frequency appears in an exaggerated form. As an instance, take intestinal worms. M. Landouzy prints seven instances in which they were the sole cause of Hysteria; but these seven are probably the whole of the cases of such a sequence that have occurred to the profession at all; and it would be a *reductio ad absurdum* to view seven and nineteen as the fair proportion of worms and uterine ailments in the etiology of Hysteria. The only collection of cases which can prove the point of the real proportion of genital cases to the whole is the record of every one that passes under observation for a definite period, whether in general or obstetric practice. M. Landouzy's statistics, however, must be held to show that the internal reproductive organs are, with more frequency than any other one part of the body, the starting-point of morbid action in Hysteria, though not than all the rest of the body together, still less than body and mind united in one view.

The assignment of the more external parts of generation as the seat of the pathological processes took its rise in a purely theoretical deduction. A cavernous tissue underlying the mucous membrane of these regions was marked off by Kobelt[1] as the exclusive home of venereal sensations: Hysteria is due according to some to lust: so in these parts it is argued must be its seat. Within the last few years this argument has been made the excuse for a most unwarrantable mutilation, the excision of the clitoris, which has prejudiced the question in the eyes of many. People are prepared to assert that sensual indulgence does not

[1] G. L. Kobelt. Die männlichen und weiblichen Wollusts Organe des Menschen und eigener Säugethiere. Freiburg, 1844.

even aggravate Hysteria in either sex, and *à fortiori* does not cause it. Now it does not require to be a physician to know that each yielding to sensuality weakens more and more the the power of control, and must therefore add to a disease whose chief characteristic is want of that power. But the power of control will not be restored by removing the organ of indulgence, any more than lunacy can be cured by chaining up the lunatic.

Those, whose experience has made them consider some special action of the reproductive organs necessary to constitute the chronic condition of Hysteria, are not quite agreed as to the date in the succession of morbid events which the action holds. Dr. Chairou, as before shown, looks upon the ovary as the "predisposing cause," and upon mental and nervous stimuli as exciting causes. Dr. Tilt agrees with Dr. Landouzy in reversing the order. They suppose the pre-existence of an undue action of the brain, which is thereby prepared to receive the visceral stimulus, the addition of which makes up the morbid state. The matter is of importance, not only as affecting the comparative value of divers portions of the treatment, but in estimating the pathology of the determining causes of the paroxysm.

This predisposing abnormal condition of the nervous system occupies a still more prominent place in the creed of those who dissent from the host of gynæcologists. Dr. Cullen indeed pronounced Hysteria a disease of the stomach; just as out of opposition M. Piorry calls it offhand "metralgie" and "ovairalgie;" but Dr. Chambers, who represents the gastric section of the present generation of dissenters, though he brings the digestive viscera to the front as potent in the causation, does not exclude other influences. Indeed he expressly designates the uterus as the next commonest cause; and with both he associates impairment of the normal balance of volition as an essential feature of the disease, and in one of the cases on which he lectures the emotional element is made the origin, the seat, and the manifestation. And in the list of remedies he places first such as tend to restore voluntary power. It is true he counts the stomach first among the visceral causes, and the most needful to be attacked with treatment; and also, by omitting instances of uterine derangement, implies that proportionally they are mere exceptions. This one-sideness is not to be excused, but it need give no trouble, if the reader will recollect that the lecturer is drawing his instances from a peculiar class, the frequenters of metropolitan hospitals, whose condition in life renders them especially liable to dyspepsia and other derangements of the digestive functions, while, by the rule of the establishment, uterine cases are picked out for the uterine wards.

Dr. Reynolds has not been led by his experience to lay so

much stress on the nutritive viscera; and he has found the perversions of function to be a consequence rather than a cause of the Hysteria; the diseased moral feeling tempting the patients to unwholesome articles of diet, in fact to swallowing things not articles of diet at all, very frequently to drinking, and to moping, all prime factors of dyspepsia. To Dr. Reynolds, whose attention has, to the great gain of the profession, been strenuously directed to disease of the central nervous system, Hysteria appears to originate in the mental functions of the brain, and to excite normal and abnormal reflex action through emotion. He would even make the visceral lesions dependent often upon the moral perversion.

Dr. Handfield Jones is still a more decided psychologist, and makes Hysteria a disease of the intellect, by reserving the term for those who need moral and not material remedies. This seems, like Dr. Chambers' way of putting the matter, an exclusiveness engendered by exaggeration in the other direction. He is so determined not to be influenced by Greek derivations that he prefers to make disease of the womb a reason for shutting the case out of his definition.

There do not seem to be in the present generation of medical men any representatives of Galen's toxæmic theory of Hysteria. He attributed the general symptoms to the retention in the blood of excrementitious menstrual matter or of semen. (We should nowadays call the latter "germ-material" in the case of the female.) To this poisoning of the blood he attributed the congestion and flexion of the uterus by the swelling of the ligaments. Yet, if there were any, they really would have not such a bad case, so often do catamenial deficiencies coincide with Hysteria, and so often does the disease cease on the restoration of full discharge.

It is quite true that in many instances the explanation may lie in the view of M. Brierre de Boismont, who attributes the uterine derangement to the nervous—*les maladies nerveuses donnent souvent lieu à des derangement de l'écoulement periodique*[1]—but he does not claim that as the rule.

Let it be remarked, in passing, that the retention of germ-material has in the female no relation at all to the influence of continence or its opposite. Ovulation, so far as it is excrementitious, is shown by the discoveries of modern physiologists to be quite independent of the male. So that the question since Galen's time is simplified, and what he looked upon as different retentions are in fact one.

The influence of defects of hæmatosis on nervous susceptibi-

[1] 'De la Menstruation.'

lity is nowhere so evident as in the action of chlorosis on the economy, and in the predisposition to Hysteria which results from this action. In the 430 hysterical cases collected by Dr. Briquet there are 152 where chlorosis existed in a noticeable degree before the appearance of Hysteria.

But, without adopting the toxæmic theory so fully as its author, there are some who do hold strongly to the very great part played by the catamenial discharge in predisposing to Hysteria. Indeed Dr. Valette (of Toulouse) whose treatise on Hysteria is highly spoken of by Dr. Briquet, actually does go so far as to suppose that the disease can exist only after previous derangement of menstruation. His mistake seems to have arisen, not so much from the speciality of his sphere of observation, as from his reckoning as morbid the accidental varieties of amount and duration of the discharge consistent with, and indeed individually characteristic of, perfect health.

Dr. Briquet in his thirteenth article, has therefore carefully brought statistics to bear on this point. He finds in 392 hysterics—

128 in whom the first advent of Hysteria had been preceded by irregularity of the function; 87 in whom Hysteria appeared before the age of puberty; 22 in whom, though the age was passed, yet menstruation had not commenced; 6 where Hysteria came on only after the change of life; 156 in whom the discharge had been neither irregular nor painful.

It appears then that in about three-eighths of hysterical women catamenial troubles may be reckoned as the possible cause of the diseased state. The number is not so great as one would or might have surmised, but still it is enough to show a connexion between them. With the exception, perhaps, of Pulmonary Consumption it would be hard to find another chronic disease so often preceded by menstrual derangements as Hysteria.

We cannot agree with Dr. Briquet in thinking this question distinct from that of the influence of the genital organs and their functions in their production of Hysteria. It is true that the genital organs are not usually the first link in the chain of causes of Amenorrhœa, still the fact of their so often coming in the circle connecting those causes and Hysteria remains to be accounted for. Dr. Briquet falls here into the style of advocacy which we have begun this article by regretting. Besides which it is not Amenorrhœa, but rather Menorrhagia and Dysmenorrhœa which are associated with Hysteria, and they are almost always due to the womb or ovaries.

Menstruation and its derangements dispose to Hysteria in two ways. In the great majority of cases it is by enfeebling the constitution and making the nervous system more acces-

sible to impressions and more liable to suffer from them. In others it is morbid action originating in divers parts of the body, in the uterus and its appendages as elsewhere, which brings the nervous system down to the pathological point.

Dr. Briquet shows by statistics that the form of Hysteria induced by the uterus is the slow chronic form, which bears to that of sudden invasion the proportion of four to one.

Under the title of "Nervosisme" M. Bouchut has proposed to separate from Hysteria those cases where there is no convulsion or globus, though there are the symptoms common to both of deranged sensibility, anæsthesia and hyperæsthesia, neuralgia, paralysis, aqueous urine, and emotional delusions. He accuses the profession of having mistaken the nature of these cases and having classed them as Chorea, Hysteria, Hypocondriasis, Mania, &c., according as their more prominent symptoms entitle them, without recognising their connection with one another. He details, generally in a vigorous and condensed form, the cases on which he grounds his argument, but he does not seem to make good his conclusion; for many of the patients have spasmodic symptoms, and one "*œsophagisme*," the difference of which from globus is hard to describe, while of several the hallucinations are so decidedly intellectual as to constitute Mania or Melancholia. M. Bouchut has, however, done good service by calling attention to an acute febrile form and stage of these nervous disorders, by whatever name we choose to designate them, and to the violent perturbations of nutrition which sometimes accompany them. He makes these the positive distinguishing marks as the others were the negative, of his "Nervosism." All of us must have met with, and been puzzled by, cases such as he describes in "*Observation XVI.—Nervosisme aigu à forme typhoïde.*" It is an ephemeral fever intensified and made serious by occurring in what we roughly call an hysterical person. It is often immediately brought on, as in the instance under consideration, by a thwarted will, and is much more common in warm countries, and in Latin and Celtic races than amongst us. And much more severe too; for in Italy hectic fever, marasmus and death after violent emotion is not very rare[1]; while with us cold-blooded creatures "a false typhoid fever," such as described by M. Bouchut, is about the worst consequence that is likely to be seen by our readers. Observation XIV is an interesting account of an autopsy

[1] Charles Lever used to maintain that assassination in Italy is a pure matter of self-preservation; unless a man can work off his steam in this way he is in danger of death by passion. And he used to relate, for the warning of friends hiring Italian servants, how very nearly his daughter killed a groom by a scolding.

of a lady who died of acute jealousy of the sitters and models employed in her husband's studio. Every organ was in a state of perfect health in spite of the marasmus which had preceded death.

For all that we are unwilling to separate from Hysteria the cases described. It is of more importance practically for a name to recall kinship than difference. The tendency to diagnosticate, rather than to group, is dangerous in the present state of science, and should be jealously guarded against. It arises from looking at diseases as a naturalist does at his specimens, in the light of positive individuals with material features of likeness, instead of comprehending their true nature as abstract negations of health, as "modes in which life is deficient." When this correct view of pathology becomes habitual, intermediate diseases and transitions from one to another seem perfectly natural, and we no longer are tempted to make new species of the infinitely varied combinations of defective vital actions in greater number than popular language supplies names for at the demands of convenience. Let the student understand that Hysteria is a Neurosis, and that "Neurosis" means a deficiency of nerve-power, and then his pathology will not be wrong whenever it shall be discovered what that nerve-power is.

The moral error of all monographists, and of monographists on Hysteria in particular, lies not so much in thinking themselves right (in which we bid them God-speed) as in thinking every body else wrong. The question about the part played by the reproductive organs in Hysteria would cease to divide the profession into parties, if every one would, like Dr. Hewitt, freely accept the experience of others and add to it his own, making allowances for the special circumstances under which each was gained. We should then probably not fail to convince ourselves that the female genitalia, while far from being the exclusive cause, far less the exclusive seat of Hysteria, still play a most potent part, more potent than any other viscus in its production.

In the first place the disease, and in the second place the normal health of these parts are influential. Their diseases are peculiarly distinguished by their length, and by the dull wearing pain they occasion, rather than by any rapid disorganization or sharp agony. Hence they act on the nervous system like moral agents. Also the debility they cause is, by the slowness of its increase, specially calculated to affect the nervous system; for we know that the tissue of which it is made is the slowest to be rebuilt by constructive assimilation of any tissue in the body. To affect it rapidly, morbid processes must be so violent as to destroy life; only those whose increase is so slow that the

rest of the body becomes inured, can injure permanently the nervous system. Again, in the physiological condition the sexual parts, though very tough and enduring of rough usage, are yet peculiarly susceptible of slight mucous-membrane-deep impressions, and the more so the healthier they are. So that there is no inconsistency in advanced lesions ceasing to cause the symptoms which had accompanied their lighter degrees.

Among the less frequent origins of Hysteria may be named enteric fever, of which Dr. Briquet has recorded ten, and intermittent fever, of which he had recorded twenty cases. Concerning the latter he remarks that in eight it had acted as predisposing (or remote) cause only, in twelve as predisposing and determining (or proximate) cause at the same time. And this leads us to make a few remarks on the application to nosology of the classifications of causes. In obscure diseases that is to say, in deficiencies affecting those parts of whose physiology we know least, it becomes impossible to estimate the place in the chain of causation which an agency should occupy, or to say of anything that it is the immediate antecedent of the phenomena we call the disease. The most practical division is that adopted by the highest Authority when speaking of moral lesions, "those things which come from within," and "those things which come from without," the *internal* and the *external* causes. There is less likely to be a mistake made by thus dividing them, and also more practical therapeutical inferences may be drawn from the etiology.

Among internal causes must be included surgical ailments, which involve a weakening discharge of pus or long confinement to bed, and which occasionally originate Hysteria. The form assumed is usually that which the profession has been made familiar with under the name of "Spinal irritation," that is, intense local Rachialgia. Hence it is not strange that among hospital surgeons should be found the principal adherents of the theory which attributed Hysteria to a morbid state of the spinal cord. The names of Teale, Griffin, Porter, and Parrish will occur to every one, and that their idea is not wholly devoid of foundation in some cases, receives support from the adherence to it by Dr. Todd, in his article in the 'Cyclopædia of Practical Medicine.'

On the other hand serious organic lesions, such as valvular disease or enlargement of the heart, phthisis pulmonalis, degenerations of the liver, or of the kidneys, advanced ovarian tumours, however much pain and debility accompany them, seem rather to exempt the patient from the nervous derangement. For it is a fact, of which memory cannot fail to supply the reader with instances, that women previously hysterical

will often cease to be so on the supervention of one of these morbid states.

In reviewing the social conditions to which experience leads various minds to trace Hysteria, due allowance must again be made for the different circumstances under which that experience was acquired. The fashionable physician finds it exceedingly prevalent among the luxurious and frivolous, and, as he is rather given to printing ink, his opinion is widespread. On the other hand, a harder life cannot be imagined than that lived by the Shetlanders, nor in some respects a healthier, for they are both prolific and long-lived. Yet in a most interesting report on their condition by Dr. Saxby, of Balta-sound, Hysteria is reckoned as the most common of all diseases. He says, "defective diet and improper clothing, together with Hysteria, which indeed is too often the consequence of one or both, appear to be the main causes of disease in Shetland."[1] "Hysteria, in every imaginable form, is common; sometimes proving fatal, when the patient, believing herself paralysed, retires permanently to her box-bed."[2] The physicians to St. Lazare, Drs. de la Morlière and Bois de Loury, find on special inquiry that more than half of those who live the wild life of common prostitutes, and afterwards come into hospital, are hysterical, only 65 in 197 not being so. While on the other side one reads of the cooped-up Ursulines, and the rigid Carmelite nuns, as peculiarly liable to nervous complaints. It was among them that prevailed the epidemics of barking, mewing, jumping, convulsions, and all the more or less singular forms of mediæval Hysteria. According to Dr. Briquet there is a great tendency to Hysteria arising out of "the family life" (*domesticité*), that is, the life led by Parisian servants—neither particularly chaste, nor particularly lascivious, neither over-luxurious nor over hard—in fact a medium between austerity and indulgence. Dr. Carrerc (de Tarn et Garonne) found in a series of 192 female domestics 65 hysterical. Dr. Besançon, of the Lourcine Hospital, found in 180 girls, mean age 22, 84 hysterical; and Dr. E. Goupil, of the same hospital, found in 52 women in the uterine ward, 23 hysterical.[3] Dr. Briquet thinks Sisters of Charity to be peculiarly exempt, but he should remember that those he is familiar with at the public hospitals are a picked lot, and that it is not likely an hysterical woman would be told off to such a post. One cannot understand the line of practice which led Hippo-

[1] Dr. Dobell's 'Report on the Progress of Medicine in different parts of the World.' 1870, p. 524.

[2] Ibid., p. 529. The said "box-beds" are an institution in Shetland; when a person's "time is come" by reason of age or sickness, the doors are closed and the patient is left without food or medicine. This in Great Britain!

[3] Briquet, op. cit., p. 122.

crates and Galen to pitch upon widows as typically subject to Hysteria; our own personal clientèle would tend to show them peculiarly free. And in the 375 cases collected by M. Landouzy, only 13 are widows, and only 14 in M. Briquet's 430.[1] Is it that at Athens and Rome the maidens were attended by the ἰητρίναι, and the married women by the μαιαὶ, the doctoresses and midwives of whom we read in Galen, so that widows only were left to the physician? Field-labour is said by most writers, probably on *à priori* induction, to exempt from Hysteria, and probably it has some influence; yet on the other hand it may be suspected that the diseases of the agricultural population are imperfectly known, *carent quia vate sacro*—because their medical attendants are not much given to writing books.

The fact is that all classes of women are pretty nearly equally liable to Hysteria, and are liable, not because they are this or that in social position or habits, but because they are women. And they are women, not in virtue of having wombs only, but also in virtue of the Creator having given them a less perfect and strong nervous system than their mates. Hence they are far the most likely of the two to fall into diseases which are deficiencies of nerve-power, just as they are the most subject to chloro-anæmia, because their blood naturally is less rich than man's in red discs, and just as men have gout and get bald, because they have less power of assimilating uric acid, and because their scalp-skin is a less favorable soil for hair than a woman's. When men have Hysteria, which is rare, an effeminacy of manner may usually be observed, which shows their emotional part to be constructed in a feminine mould.

The reader may ask on what grounds muscular movements, often of great force, and secretion often excessive, are attributed to deficient nerve-power? This is not the place to go into the question of the "Dynamics of Nerve and Muscle" at length, and we will content ourselves with observing that nerves are fed with blood, that the pale skin, low temperature, cold extremities, and weak pulses of the hysterical show how sluggish their circulation is, and hence how slowly the nerves are renewed, how little work they can do in a given time. As Galen says, the hysterical are like hybernating animals, from the imperfect aëration of the blood, and consequent fall of temperature.[2]

Control and direction by volition is the hardest work the nerves have to perform, and for its performance they require to be most perfect. Reasonably, then, when controul is seen to be

[1] And of these last, six ought not to count, for the disease came on the day of the husband's death, and is attributable to moral not physical causes.

[2] Loc. antea. cit.

wanting, we infer that the violent motions are the unbalanced ebullitions of a weakened agent.

Hysterical spasms are sometimes called "spinal" or "reflex." Such does not appear to be their true character. Reflex movements are peculiarly constant, and their form is attached to the race rather than to the individual. Pinch the toes of any decapitated frog and the foot is retracted always in the same way. Tickle a man's nose and he sneezes; put your finger to the back of his tongue and he retches, lower down and he swallows. You know exactly what will happen in every animal of the species. But it is not so with the movements characteristic of Hysteria. When Dr. Tilt pressed the left ovary of his subject, unconsciousness followed; when M. Chairou did the same to his there were the convulsions, &c., which alone he calls by the name of Hysteria. In Schutzenberger's three cases there would appear to have been in the first, eructations and globus; in the second, epigastric constriction and loss of consciousness: in the third, suffocation, wry faces, convulsions, and stiffening of the arms, each set of symptoms arising from either an accidental or a manual compression of the ovary.[1] And it is the same with various diseases which result in Hysteria; one can never predict beforehand what form of it they will induce, even in the same person, much less in the same class of persons. Moreover, reflex acts can be restrained only by a counteracting palpable muscular effort; whereas these emotional movements can be curbed by mere volition. Something ridiculous occurs on a grave occasion; we feel a laugh coming, we determine to curb it, and do so without stirring our countenance; whereas a cough or a hiccough can be stopped only by heroic contractions of antagonistic fibres.

Darwin had an ingenious theory for explaining instinctive motions, both normal and morbid. He thought that there is an accumulation of "sensorial power," either by a muscle not being used, or from the antagonistic muscle being over-used, and that hence pain is consciously or unconsciously felt in a part, and that the motions are an effort to relieve the pain.[2] There is a justification of this theory in the satisfaction experienced after both reflex and emotional acts—say a sneeze, or a yawn, or an angry stamp. The hysterical often have pleasurable sensations follow on phenomena of a far from pleasurable nature, such as vomiting, for instance;[3] and they can hardly

[1] "Recherches sur les Causes Organiques des affections Hystériques." 'Gazette Med. de Paris,' 26me Sept., 1846.

[2] 'Zoonomia,' vol. ii, p. 142.

[3] See a remarkable case recorded by Pinel ('*Dictionnaire des Sciences Med.*,' art. "Spasme") of a medical man who described his vomiting as a "voluptuous ecstacy" and an "ineffable enjoyment."

be persuaded that a good cry or scream is not to their future advantage.

Is Hysteria hereditary? Doubtless hysterical persons have hysterical parents; but is the disease transmitted by descent? Many centuries ago Pelagius set the Christian world at variance by discussing whether original sin consists in "the following of Adam," or whether it is the innate "corruption of every man that is engendered of the seed of Adam," as the Anglican Articles word it. And the same question is by no means decided in respect of Hysteria. Much more than the mere transmission of a germ is involved in having an hysterical mother; bad habits may be imbibed by the eyes even while milk is being drawn in by the mouth. We cannot attribute the full importance which M. Briquet does to the statistics he has collected of the family antecedents of 351 hysterical, and 167 non-hysterical persons. He details the pathological history of the parents only; whereas, to give them any decisive value, instances, direct and crucial, of atavism ought to be recorded—cases, where the parents being healthy, the grand-parents or collateral ancestors were hysterical. The medical attendants on historical aristocratic families might contribute useful examples by the aid of the printed pedigree. And valuable information might be obtained from the Foundling, where the mother, though entirely separated from her offspring, is yet well known, and usually of somewhat superior intelligence.[1] These would help to decide whether the heir-loom is from within or from without.

The worst of the theory of heredity is that it tempts to shifting on to the Creator the responsibility which we, at least, ought to share.

The subject is thus brought from the internal to the first and most serious of the external causes of the disease—Imitation. Some cases doubtless are mere shams, and begin and end wholly at the will of the shammer. No further notice need be taken of these, except to remark that the deception may generally be detected by the movements not exactly copying the emotional characteristics of the real disease. Transitional cases from these are others where there is a motive, but yet an unhealthy want of power to resist it till a stronger motive is supplied; such are the epidemics which have been checked at

[1] The question is a good deal simplified by one fact which comes out of M. Briquet's statistics, viz. that the influence of the father is almost nothing. An important negation. This agrees with what M. Baillarger notices in respect of insanity, that its transmission by the mother is twice as often to the female as to the male descendants. (Baillarger, 'Rech. Stat. sur l'heredité de la folie.' 'Annales Méd. Psycholog.,' vol. iii, p. 328.)

once by threats of stripping, whipping, and hot irons; as in a convent full of naughty school girls and nuns, who persisted in mewing, till the lady superior called in some soldiers ready to administer corporal punishment in military fashion (*Nicole in Briquet*). It would be unwise to copy these forcible measures, not only because they are illegal, but because there is some danger of their being disgraced by failure. The prospect of being burnt alive only made witches more infectious, and the poor possessed jumpers of the Cevennes more indecently blasphemous. Only very early in the day will threats stop the spread of the epidemic. Even where the initial morbid act is wholly voluntary, it may be performed by a person with an hysterical diathesis, and then arrest ceases to be under the control of the will very shortly, and each successive act further removes it. It seems in accordance with the analogy of nature that, even in a non-hysterical person it should, though of course with more difficulty, generate the diathesis; for it must be remembered that morbid processes are not of another nature from physiological processes, but are exaggerations, misplacements, or perversions of them. We all know the stale trick of making a company yawn by showing the picture of a yawner, and how a cough in church will set half the congregation clearing their throats. Hysterical imitation is an identical phenomenon.

There is nothing to be gained by calling the symptoms "hysteriform" instead of hysterical. They are of exactly the same nature as those of uterine or other bodily origin. A man is just as truly hung if he arranges the preliminaries himself as when they are against his consent.

M. Cérise, a valuable writer on the educational aspect of these questions, maintains that "the convulsive expression can be imitated only with the condition of the production of a visceral derangement analogous to that which produces the sentimental expression; there must be at work, over and above this purely external imitation, an imitation altogether internal, mysterious, invisible, but real and incontestable."[1] If this is anything more than a speculative platitude, it means that similar external acts proceed from identical visceral causes. But from the example of the artificial production of Hysteria in several forms by acting on the same part, it appears that the same visceral cause originates variable phenomena, and, consequently, that similar phenomena may proceed from different causes.

With respect to the influence of the Passions and Affections,

[1] L. Cérise. 'Des fonctions et des maladies Nerveuses dans leur rapports avec l'Education,' p. 458.

Dr. Briquet justly remarks that it is not solely in proportion to their strength that they act in the production of the hysterical diathesis. A course of happiness, pleasure, mental or physical, never does so, he says. Certainly in the 430 patients whose cases he has taken, the moral predisponants were all of a painful nature; but it must be remembered they were all of the lower, or at least the hard-living class, and that possibly statistics collected among such as live at home at ease might show another result. Musical people tell us that their peculiar enthusiasm is the highest pleasure mind or body can enjoy, the exterior gravity of expression it induces being very properly explained by Shakespere's Lorenzo as merely the effect of attention. Yet it certainly does predispose to Hysteria, as well as excite the paroxysms. Coarser gratifications of the senses so soon sear the taste, and cease to be pleasures, that their effects are limited to those secondary on exhaustion.

The order of frequency of the causes of sorrowful emotion in the 430 was as follows:

1. Bad treatment by parents or husbands.
2. Family quarrels.
3. Financial difficulties and loss of station.
4. Nostalgia.
5. Jealousy, continued fear, weariness of soul (is not this last an effect rather than cause?).
6. Disappointed love in a very small number of instances.

What a shock to one's romance, that the least selfish of the painful passions should be the least common! In the name of all that is poetical it is to be hoped that in affectionate England a higher stand might be allotted to it. Had the number of instances not been so very small, one would have been glad to know the proportion of the different sorts of love. In Shakespere's one example of an hysterical woman, Constance in King John, it is maternal love that is blighted; and the experience of the writer of this article chances to find that a commoner cause than thwarted sexual attraction.

The influence of wearing bodily pain has already been spoken of. No critical remarks concerning its relation to the contemporary aspect of the subject require to be made, except to draw attention to the removal in the new Nomenclature into a separate category of what has been formerly called *clavus hystericus*. It now comes indeed close to Hysteria, under the name of "Hemicrania,"-a variety of Neuralgia; but to the College of Physicians the only meaning of Clavus is a "corn" or "bunion." And we think the College is right, for often as we have seen the pain in question precede Hysteria, and in fact, occupy the apparent position of a cause, we have so

seldom seen it follow convulsive phenomena, that the coincidence appears accidental. The headache really symptomatic of Hysteria is not *clavus hystericus*, but a generally diffused pain in the muscles and fasciæ of the scalp, with a certain degree of tenderness on superficial pressure. In short, this, as well as other hysterical pains, has not the nature of Neuralgia so much as of Hyperæsthesia; the pain is elicited only by the application of a local stimulus, though it may continue after that stimulus is removed. This is very demonstrable in the skin, but in the internal viscera the stimuli are so often physiological that it is difficult to identify them.

Hyperæsthesia is peculiarly a symptomatic condition, and unlike Neuralgia, never need be designated as primary, as the Nomenclature rightly indicates by a footnote. Ninety-nine times out of a hundred the disease of which it is symptomatic is Hysteria.

According to M. Briquet's statistics, the most frequent seat of the over-sensitiveness to pain is the muscular tissue. Nineteen-twentieths of the 430 tabulated suffered in this fashion. His remarks on the pathology of this fact are so good, that we venture to translate them at length.

"It is easy to explain the frequency with which the muscular system is deranged in hysteria. The muscles are the organs of expression; the tone of the voice, gestures, and postures of the body are muscular acts. Let a person be the prey of violent passion, and see how the throat contracts, how the larynx is so painfully convulsed that screams won't come out, how the pectoral and epigastric muscles are so straitened as to stop the breath, while the limbs are driven by a feeling of discomfort, or of cramp, to throw themselves about. Familiar also to everybody are the tremblings and exhaustion which follow, and the weariness and bruised feel in the muscles. These painful sensations, by repetition, end in becoming permanent, and passing from the physiological condition to the pathological.

"Now look at what happens to an hysterical woman." * * * *

(The reader's memory of the last fits he saw will save the translation of a half page.)

"Tough, indeed, must the muscular machinery be if its sensitiveness is not raised after such shocks."

The diagnosis of this myalgia from neuralgia is made by observing that the locality of the pain is not governed by the trunk-nerves, as is the case of the latter disease. We have always found this sufficient, but M. Briquet adds another mode, viz. the effects of faradisation, which can be used in doubtful or severe cases. A healthy muscle is only uncomfortable from the passage of an interrupted induced current of moderate intensity; but to the myalgic fibre the same amount is agony, followed

luckily by rapid relief. The reader must be prepared for the patient not liking this means of examination, unless it can be truly represented as curative.[1] Faradisation affects neuralgia very slightly, if at all.

To the muscles of the part alone Dr. Briquet's statistics refer the pains in the epigastrium which hysterics suffer, in more than one third of the cases where this symptom is noticed. The other two thirds presented at the same time derangements of digestion proper to true gastralgia. Ten only out of 317 had gastralgia alone without epigastralgia; and 31 out of 358 were free from pain at the pit of the stomach altogether.

Guided by this investigation Dr. Briquet was led to question the pathology which refers the well-known pain in the left side of hysterics to intercostal neuralgia. He observed that it does not follow the line of the nerve-branches, but occupies the area of the muscles, and is most intense at their insertions. Again, that it is felt only at the point of pressure, and does not radiate in the direction of the neural trunks. Again, there is never any corresponding pain at the issue from the bone of the nerve-trunk, as is the case in neuralgia. For these reasons, and a few others of less weight, he looks upon the pain in the side as myalgic, and not to be confounded with "Pleurodynia," as the College of Physicians denominate intercostal neuralgia.

This pain in the side is interesting not only from its extreme frequency and severity, but because it is a marked feature in some fatal forms of Hysteria. Dr. Addison records an instance of death in an hysteric fit, where a marked feature of the case had been infra-mammary pain to an excessive degree.[2] A careful post-mortem examination showed no lesion except a slight redness at the entrance of the stomach, which can hardly be taken as evidence that that part was in a state of disease. In to-day's paper there is a description by Dr. Livingstone of eight deaths of healthy negro slaves from acute Nostalgia. They made no complaint but of pain in the cardiac region, on which they pressed their hands. And they did this not from any notion that there is the seat of sorrow; for, as the Doctor says, they believe the heart to lie behind the upper part of the sternum.[3] He adds:—"This to me was the most startling death I ever saw. They evidently died of broken-heartedness,

[1] Apropos of this he tells a capital story. A girl, ill-used by a brute, sobbed and cried for several days, and then swallowed some lucifer matches. She put herself under medical care with agony in the epigastrium, apparently the result of corrosive poison. Faradisation first aggravated and then entirely cured the pain, showing it to be muscular, and saved the patient from the orthodox treatment of suicidal attempts.

[2] 'Observations on the Disorders of Females, &c.' London. 1830.

[3] 'Daily Telegraph,' July 29th, 1872, from 'New York Herald.'

and the Arabs wondered, seeing they had plenty to eat." The plain pitiful tale is made to a physician still more dreadful by the knowledge that in severe Hysteria the death is often only apparent, and that many of these homesick victims of commerce are probably buried alive. Is it possible that in intense acute cases the cardiac muscle becomes affected along with the more superficial? The idea is suggestive of the use of faradisation through the heart to preserve life in dangerous Hysteria and Catalepsy.

Hyperæsthesia of the organs of the special senses appears in the ordinary use of them being painful. This is familiar enough. But M. Monneret, and after him M. Briquet, believe in an exaltation of their powers, which jars a good deal with our notions of how disease affects the higher manifestations of life. The former quotes the case of an hysterical woman who heard her husband's step in the *porte cochère* long before the rest of the company; and the latter that of two girls who read with the eyelids apparently shut. These are simply the results of attention and practice. This line is written with the eyes apparently shut; and if they will take the trouble, the printer can compose it, and the reader catch its meaning, without showing any eyeball.

It is certain that in Hyperæsthesia of the skin accuracy of touch is generally lessened, and never, so far as we have observed, increased. It is hard to conceive the organs of special sense being governed by a different law.

Anæsthesia, complete or partial in localised spots of the surface of the body is, most probably, much more common than most of us know. We do not, like Hopkins the witch-finder, explore with pricks and pinches the whole person of a victim, or we should be more familiar with these *stigmata diaboli*, which have condemned many thousands of poor creatures to the stake. M. Gendrin in 1846 found them so often that he claimed them as constant phenomena and diagnostic marks of Hysteria. From M. Briquet's numbers, however, they would appear to be absent in two fifths.

M. Chairou has directed attention to one form of Anæsthesia, which he has found so often that he has set it down as a pathognomonic sign, viz., insensibility to the reflex action of the epiglottis. You introduce a finger quietly and gently into the fauces, scratch and tickle the epiglottis, and if the patient does not retch the case may be set down as one of Hysteria. It is certainly a feature in some instances, and there will be no great difficulty in investigating its degree of prevalence. Another common Anæsthesia is insensibility to tickling the soles of the feet, or rather the sole of the foot; for the sign is most distinctive if one foot, preferably the left, be affected.

We should be glad to turn the attention of our reader to the inquiry—What relation M. Chairou's symptom bears to so-called hysterical vomiting. What is "hysterical vomiting?" Dr. Tilt warns one not to apply the term when it arises from lesions of the reproductive organs. Truly enough vomiting, real vomiting, occurs not only with uterine disease, but with displacements—even temporary physiological displacements—of the womb, as in pregnancy in non-hysterical persons. When, then, it is found in hysterical persons along with the said lesions, we think it ought certainly to be attributed to them, and not to the Hysteria. The more so because the rejection of food, genuinely due to Hysteria, is not vomiting in a strict sense; the greater part of what is thrown up has never been in the stomach at all, having none of the smell, taste, or chemical reaction peculiar to that cavity. It comes up easily, immediately, and by a reflux more akin to rumination. Only consecutively, after straining, do the acid contents of the stomach appear. Hence, we have suggested the inquiry whether it may not depend on a sort of anæthesia or paralysis of the gullet.

The pathology of hysterical paralysis is made by M. Briquet so much the same as that of hysterical myalgia, that is to say, an exhaustion of fibre force by unwonted strain, that its repetition is unnecessary. It is a plausible explanation, but still one is tempted to ask, if they arise in the same way and from the same cause, why do they occur so rarely together in the same muscle? Moreover, M. Briquet's own statistics show that only in about half of the cases of paralysis has it followed on strong convulsions. In a real paralysis from over-use—scriveners' palsy—the cure is always slow and steady, in hysterical paralysis it is sudden and irregular.

Before leaving the subject of hysterical paralysis, let it be remarked that, though it was noticed by the ancients, the attention of the modern profession was first called to it in 1838 by Dr. John Wilson of the Middlesex Hospital.[1]

Of course it was understood, when the causes of Hysteria were discussed, that the causes of the permanent morbid condition were spoken of, not of the paroxysms. Respecting the latter some monographs enter into long and elaborate recitals of the way in which hysteric fits are brought on. We are disinclined to analyse or criticise these, because the whole pathology of the matter may be summed up in a very few words—a fit may be brought on by anything whatever which makes a *sudden* call on mind or body for action. Let the reader test this by any case he sees or reads about, and he will acknowledge its

[1] 'Med. Chir. Transactions,' vol. xxi, p. 107, "On Nervous Affections peculiar to Young Women."

truth. A patient will go through a surgical operation without flinching, because previously prepared, and then will fall into screaming and sobbing and choking at an unforeseen pinch. Daily moral martyrdoms are borne by the same person who will be completely upset by the unlooked for thwarting of a whim. Touching an ovary will cause a paroxysm, a tumour will not.

The opinion of each pathologist as to the frequency of the different sorts of sudden call depends very much on the window from whence he has studied life. M. Louyer-Villermay, who seems to have practised mostly among what is now called the *demi-monde*, finds the passions of love or lust the most common. To those who attend convent schools the excitement of confirmation and initiation occupy that position. The accoucheur thinks manual examinations of the private parts, cauterizations, pessaries, &c., to be at least very frequent excitants of a fit. While to the hospital physician such causes do not occur, and frights, scoldings, injustice, venesection, wounds, and accidents, take their place in his mind.

To sum up the conclusions to which a ramble through the most recent interpreters of the patholology of Hysteria may lead—

1. Hysteria is many-sided, and the greater number of those who study its aspects are prevented, by various social and other circumstances, from seeing more than one side.
2. Hysteria is a functional disease of the nervous system in which the actions of mind and body are involved.
3. It may originate hereditarily or idiopathically from either mind or body.
4. It originates in the female more frequently from the genital organs than from any other one part of the body.
5. The mental causes of the diathesis are sorrowful emotions, imitation, and habit.
6. All conditions of life are nearly equally prone to the disease, which owes more to the diathesis than to external causes.
7. The paroxysms are due the same causes as the diathesis, but suddenly applied.

The TREATMENT follows on the PATHOLOGY. True especially of Hysteria, which is so varied in its manifestations in different individuals, that empiricism, at least rational empiricism, can draw no conclusions.

First, of the first point whose modern aspect was criticised—viz., the connection between Hysteria and the genital viscera. Does it not seem obvious that specialists should still continue to treat by local remedies the cases which are distinguished by local lesions of tissue? These inevitably present derangements of functions which cannot fail to attract attention. Again, is it

not clear that where there is no derangement of function, or only secondary derangement of function, the more strictly the parts are let alone the better? When there is a cold in the head one does not put remedies up the nostrils, nor medicate the rectum for a diarrhœa. With all his predilections for ascribing an uterine pathology to Hysteria, Hippocrates strongly deprecates local interference. "Touch not the womb, and give no drugs," is his sage advice[1] respecting the treatment of hysterical old maids. It is certain that many hysterics are made worse by the emotions of disgust, fear, suspicion, or prurient curiosity excited by examinations. On this ground we protest most strongly against the dogma set forth by M. Chairou, that "the treatment of Hysteria ought to consist above all things in the treatment of the ovaries;" the more so when we find that this treatment is (in his text, not in the cases) leeching and blistering as near as possible to those organs, preferably on the cervix uteri.

The next practical point is the influence of Imitation. It is powerful for evil, and therefore may be made powerful for good. The children of hysterical families should have an education as like that of Quakers as is consistent with the avoidance of eccentricity, with some additions which will be suggested. Calm, sober habits; calm, sober people about them. It is impossible to agree with M. Briquet in recommending a home education. The mixing with young people of equal age necessarily involves self-control, and a thoughtfulness of others and of the external world, most useful in the cure of Hysteria.

In Mr. B. Carter's valuable little book on the pathology and treatment of the disease, one of the prime things insisted upon is, that "no moral treatment can be effectually carried out so long as the patient remains in her own home."[2]

And we would suggest that education and amusements may be so arranged as to make active imitativeness bear good fruits. The mere fact of conscious imitation (provided always it is not an imitation of Hysteria) seems to be curative in its influence. A popular dramatist informs us that his wife is a martyr to Hysteria whenever she has a holiday of any extent, but no sooner does she get into regular harness, spending her nights in a mechanical imitation of the peculiarities of others, than the nervous derangements all vanish. Posturing, drilling, riding, swimming, games of bodily skill, recitations, acting, reading out loud, dancing lessons, music lessons, may have a great deal of the good they

[1] *πρὸς δὲ τὰς ὑστέρας προσφέρειν μηδὲν, μηδὲ τὸ φάρμακον πίνειν.* He advises marriage, a glass of spiced wine before dinner, and the avoidance of scented hair-oil. 'Hippoc. de Nat. Mul.' (near the beginning).

[2] Page 105.

effect traced to the rhythmical movements, the voluntary attention, and the self-controul they demand. They may be employed to mollify agreeably the grimness of the educational schemes proposed by French writers.

"Music lessons" are mentioned as beneficial, though music a few pages back is accused of causing the paroxysms and aggravating the diathesis. Now music implies two things in very variable proportions, an expression of the feelings, and a rendering of those feelings into a peculiar language of rhythmical sounds. The first is noxious, the last curative. A few years ago we were consulted by a pianist, an ardent lover of his art. When he was giving a lesson to stupid pupils, however vexed he grew, he preserved the composure of the *maestro;* but when they were appreciative and sympathetic, his enthusiasm bore him away—his brain and his instrument were one, together "untwisting all the chains that tie the hidden soul of harmony." Then his arms stiffened, his cheeks glowed and paled, a cold sweat wet him through, he choked, made horrible faces, could not speak, sobbed and cried. He was obliged to give up his profession, and since he has become a city clerk has had no more hysterics. If, then, any hysterical patients have the rare gift of genius, and music is a real language to them, by all means let it be interdicted. But to the average young person it means only tapping certain keys when certain marks are read in regular order, and taking care that the tones of the voice are the same as the tones of the instrument, the goal of ambition being a merited "very nice." Let it be bestowed freely, there cannot be better discipline.

Which condition of life is more suitable for the patient, continence or the contrary? is a question which has been asked in reference to the treatment of Hysteria from the earliest times, and seems as interesting as ever, to judge by the many pages it fills in monographs. Of old it was decided in very trenchant fashion: Hysteria is an abnormal state of the genital organs arising from their not being used, so of course continence is bad and indulgence is good, and no more need be said about it. But of late years people have begun to observe that the disease is really not more prevalent among continent persons than among the opposite, provided they are both placed in the same circumstances in other respects. Of course it is common among maidens, because the greater number of women between 15 and 21 are in that condition, and it is at such an age that there is the greatest liability to the complaint; and they are also idle, or overworked, unprotected, ill-treated, badly fed, and exposed to bad example more at that age than any. But if we take a batch of well-to-do well-employed old maids of from 30 to 50,

there really is no more Hysteria to be found amongst them than in an equal number of matrons. Writers often hint, in a mysterious way, that there are secret histories to account for the hysterical wives, proving the evil of continence. But over and over again one sees these hysterical wives become happy mothers and remain hysterical; and, on the other hand, wives whose reproductive organs have lain dormant, and who are not more hysterical than the average. It is of course difficult to collect in a single experience numerical evidence on a subject about which reticence is usually observed; but we have no doubt that, if he sets aside his foregone conclusions, the reader will be able to cap from his own observation the following instances in point:—(1) A lady, who had an hysterical mother, according to her statement, and a brother once under our care with nervous satyriasis, and may therefore be reckoned very prone to nervous derangements, married an impotent man and remained a maiden till she became a widow. She has never been the least hysterical, though she protests against being considered what she calls "a cold creature," and appears to be much like other women, except a mitral valve slightly defective. (2) A young gentleman, impotent from hæmorrhoids and over-anxiety, came under our care a few years ago. He had married, and his wife retained the complete signs of virginity after more than three years of wedlock. She was loving, impassioned, and anxious for children; but hysterical she never became until after her husband's cure, when she had a severe confinement, and has since (we understand) got invalidish and nervous. (3) A draper's wife, in her first essay at child-bearing, nearly died of hæmorrhage. Her husband, afraid of losing an excellent woman of business, has not come near her since. She is not hysterical, and he suffers only from flatulence. (4) A clergyman, on mistaken religious grounds, abstained for several years from marital intercourse. The wife did not become hysterical, and he only had gastralgia, which seemed mainly due to fasting. Cases where continence is advised by a physician do not prove much; because, in the first place, there is always a suspicion of its absoluteness; and secondly, the couple are borne up by hope of better times, a very antihysterical feeling. But the four cases are not balanced by any *per contra* as yet, and are sufficient to justify an agreement with St. Paul, Augustine, Epictetus, and many others not ignorant of human nature, that the effect of continence is increased self-control, and consequently increased power of resisting Hysteria. A single life may then safely be sanctioned for persons afflicted with the disease, if other circumstances render it advisable.

The marriage question is distinct from that of continence

or its negative. Matrimony involves a good many things besides the engendering of offspring. About marriages of affection a doctor is not likely to be consulted, except as a friendly compliment, by wise people; to the unwise, a loving guide may be safely recommended; in both cases a hearty consent is the obvious course. But to take a husband or wife, as one buys a drug, for the cure of sickness, shows a degraded view of life, and, moreover, is apt to be unsuccesful. In 98 cases investigated by M. Briquet wedlock appeared injurious in 50; without influence in 31; curative, or at least decidedly ameliorative, in only 17. Marriages are in France so generally sexual unions arranged by the parents that it is to be feared the majority were of that sort.

The effect of sudden shock, however slight, in inducing paroxysms, ought to give some hint of value to therapeutics. Should all risk of thus inducing manifestations of the disease be avoided, lest it be thereby aggravated? The result is increased sensitiveness. Better it is to inure and educate the nerves to bear gradually easier and easier the inevitable. To this purpose nothing conduces so well as rapid alternations of cold and heat through the convenient medium of water, hot, cold, in vapour, and iced. Among the various well-known modes of applying this remedy it is curious that French authors omit our familiar friend the shower bath, which is the most efficacious of them all. The omission of the douche is less surprising, on account of the difficulty of introducing it into private houses.[1]

While on this subject, it is as well to warn Englishmen that "hydro-thérapie" in French medical works does not mean what we technically call Hydro-therapeutics, or the going to water-cure establishments, or anything still more novel. It is in fact nothing new at all, but simply bathing; and Pharaoh's daughter, Nausicaa, Queen Pomare, and their dusky flocks of merry maidens, were all unconscious "hydro-therapists." It would be more effectual in this country than at present if, after the natural fashion of the above-named heroines, and of the modern French, we made it more of a relaxation and less of a business.

It is well worth while to explain to intelligent patients the principle on which the shower-bath is ordered, and why we give them the trouble of undressing for it several times a day; for then they will understand the reason for opposing, in other ways also, their coddling propensities, and for urging them to do many things indubitably painful.

[1] The best substitute is the "gardener's bath." Set the patient on a stool in a big sponge bath with a screen round it, and then bring a hydropult or garden engine to bear full on the spine, or elsewhere, as required.

It is pleasant to see that M. Chairou and M. Briquet, though they differ on so many points, are quite agreed as to the beneficial effects of opiates, and especially of opium, in Hysteria. It is certainly very successful, and it is difficult to explain its success in any other way than by supposing it to restore the balance of the nerves and their excitants by arresting the activity of the latter. A nightly dose of opium may be continued for months with nothing but advantage arising from it.

The treatment of hysterical Anæsthesia by Faradaic electricity does not seem to have been systematically employed in England. Its success in the practice of M. Briquet will make our not trying it unpardonable. His account is of the most *veni-vidi-vici* kind. A lady comes to him stone deaf of one ear; and the lobe was insensitive. "Go," says he, "over the way to M. Duchenne, ask him, with my compliments, to cure you, and come back in ten minutes." At the time appointed she returned, hearing perfectly. The action of electricity is not yet understood enough for an explanation of this to be attempted.

During spasmodic paroxysms it is the usual custom nowadays to loose the patient's clothes, and leave them pretty much to themselves. The old fashioned plan of tying bands round the limbs seems to the present generation to savour too much of the straight waistcoats and fetters, of which we have morally made bonfires. It is possible we are erring on the side of too much liberty, for the pathology of the disease leads to the impression that each unrestrained indulgence aggravates it. MM. Georget and Foville lay the patient on a bed, put an attendant to hold each shoulder, and two more to the legs. This requires a larger staff than can generally be got; and a readier, though rougher, method obtains in some parts of England, of placing a solid person to sit on the stomach, which as effectually stops motion as sitting on the head of a fallen horse. It seems to bring back the diaphragm to voluntary composed action. A similar effect is produced by holding the patient's mouth and nose, as recommended by Dr. Hare, till she is nearly stifled. A strong voluntary effort of inspiration is made, and the fit passes off. Both are more powerful than the conventional dash of water in the face.

The slight connection between hysterical irritation[1] and the nutrition of the body, except as a remote and exceptional consequence, leads the thoughtful physiologist to anticipate little help from tonics. But it is to be feared medical men in general

[1] Irritation = "a disturbance in the endowments or functions of a part independent of either actual inflammation or organic lesion." (ADDISON.)

are not thoughtful physiologists; for still more necessary now than in 1830, when it was given, is the warning of Dr. Addison, that their results are very unsatisfactory. He says, "In the highly irritable and susceptible state of the body at large, and of the alimentary canal in particular, the more stimulating or irritating tonics cannot be borne, creating sickness, pain or uneasiness at stomach, loss of appetite, headache, and divers unhappy sensations not easily defined by the patient. Chalybeates not only offend in this way, but are extremely apt to excite the uterus, so as to produce excessive menstruation, &c."[1] The warning does not seem so much wanted in France, for M. Briquet does not allude to the use of tonics, except where there is chloro-anæmia.

It is the fashion now to ridicule the long list of diffusible stimuli employed by our fathers, and much second-hand learning and bad spelling is thus displayed. Of course the list was long, for the patient was not cured. But does not its length show a craving on the part of both doctor and invalid for something of the sort? The rapidly growing disuse of these nauseous stimulants, to the amount of which there was always a limit, has been accompanied by a corresponding addiction to sweeter poisons, unrestrained by any bonds, human or divine. We have known an hysterical clergyman's wife steal the sacramental alms to buy liquor, and a lady of genuine piety tell lies to hide a brandy flask in her reticule. Neither were dipsomaniacs; both took by medical advice, for "nervous debility," the dietetic stimulant which a century ago would have been rendered needless by assafœtida, "*stercus diaboli*," or some equally untempting drug. Our generation may laugh, but does it win? Whatever can be got to take its place, alcohol in a pleasant shape must not be permitted to hysterics, or the sooner medical men abdicate their posts as guardians of the public health, the better.

How is an hysterical woman to leave it off? The answer we would at once and unhesitatingly give is—by never once taking it again. A terrible struggle, but not dangerous to health. A medical man who has not tried will be surprised to find how often Hysteria and invalidism, which has lasted for years, will vanish before the face of abstinence, and how seldom it is necessary to have recourse to the excitants of the Pharmacopœia.

[1] Op. antea. cit., p. 56.

V.—Scottish Meteorological Journal.[1]

THIS publication, the mouthpiece of one of the most active and independent associations in the country—the Scottish Meteorological Society, continues to maintain its eminently practical, and therefore useful character. Since the journal was last noticed in the pages of this Review (in January, 1870), the science of meteorology has made considerable advances, and we are glad to be able to record that the representative bodies of that science are now much more disposed to work in unison than was the case formerly. This augurs well for the future; so that we may with confidence look for further and rapid progress at no distant period.

One branch of meteorology—the one, indeed, which most nearly concerns our own profession—has hitherto received but scanty attention at the hands of most investigators. We refer to medical climatology, a subject which, notwithstanding the researches of Scoresby-Jackson and others, is still involved in very great mystery, and the investigation of which is beset with no ordinary difficulties. It is only by the collection and intercomparison of numerous observations made in all the habitable parts of the globe that anything like a system of climatology can be worked out.

For this reason a paper contributed by Mr. C. Home Douglas to the thirty-second number of the journal demands especial attention. Mr. Douglas writes on the winter climate of Malaga, where he resided in charge of an invalid during the winter of 1870-71. To intending contributors to the subject of climatology we should commend the course pursued by the author, that, namely, of keeping a meteorological journal, and at the same time noting the effect of all changes of weather on different classes of invalids. Thus only, as experienced climatologists know, can reliable conclusions be arrived at. Mr. Douglas gives a pleasing account of the Malaga winter. In January, which is the coldest month, the mid-day temperature in the shade is usually about 58° or 60°. In 1871, the month named was rather colder and more broken than usual—February 4th was wet, "but from this date to 22nd February nothing deserving the name of a cloud was seen on the sky." The weather during this period was still warm both by day and by night. But at times, especially in January, there came days with a scorching sun and a comparatively cool or cold atmosphere. Then the patients should stay indoors, while on a dull day, the temperature being 60°, they might with impunity take open air exercise.

[1] *Journal of the Scottish Meteorological Society.* New series, Nos. xxx—xxxiv inclusive, April, 1871, to April, 1872. Pp. 144. Quarterly.

Mr. Douglas attributes the hurtful effects of the former type of weather to the *low temperature* of the air. We are not sure that he is quite accurate in this. On such a day as he describes, with hot sun and cool breeze, the per centage of humidity is invariably very low, and there is little doubt that to the dryness, rather than to the mere coldness of the air, the intolerance of such weather by invalids is to be attributed. The great advantage of Malaga as a sanitorium in winter consists in the possibility of taking frequent exercise in the open air, as cold and broken weather is quite exceptional, but with this its measure of praise is complete. "Except its climate," says Mr. Douglas, "Malaga has little to recommend it. It is a dull, dirty town, pervaded by evil smells, inconceivable in variety and intensity. There is, however, at least one pretty drive or walk—that towards Granada. Here one soon gets among the hills, in spring pink and white with the blossoms of the almond trees. Groves of orange and lemon, with troops of swarthy black-eyed peasantry on mules and donkeys, with tinkling bells, may afford a pleasing novelty to the eye of the British invalid."

In the thirtieth number Dr. Arthur Mitchell, Convener of the Medico-Climatological Committee of the Society, quotes a very valuable communication on "Death Rate and Temperature" by Mr. Gibb of Kettins, Forfarshire. That observer compared the lists of burials registered in the parish of Kettins in 108 years at intervals from 1686 to 1855 with the mean temperature of 13 years according to months, and the well-known inverse ratio of deaths to temperature is as remarkably exemplified in this case, where (as Dr. Mitchell says) *small population* and *long time* are dealt with, as in those cases where *large population* and *short time* have been dealt with. The monthly mean number of burials, calculated from 108 years, falls gradually from 2·81 in January to 1·41 in August, the respective mean temperature of those months being 36·1° and 56·5°.

A "Report on Ozone Observations" in the thirty-third and thirty-fourth numbers may be accepted as containing the latest and fullest information on an obscure subject. On January 14th, 1869, the council of the society appointed a committee, consisting of Dr. Arthur Mitchell, Mr. Thomas Stevenson, C.E., Dr. James Sanderson, and Professor A. Crum Brown, to draw up a report on ozone and the best methods of observing it. To some of the results obtained we have already drawn attention on a former occasion, but the present report deals more especially with a series of observations made at the Royal Botanic Gardens, Edinburgh, in the summer of 1869, the expense of the experiments being defrayed by the Marquis of Tweeddale, President of the Society. In all the experiments aspirators were used, a

known quantity of air being drawn over the test-papers within a given space of time. The results, though more definite than any previously obtained, are yet far from being quite satisfactory. The aspirators, too, are open to improvement. We may here mention two of the most marked results of the experiments. First, it was found that with south-east, east, or north-east wind the quantity of ozone was very small; secondly, most ozone was found to be present, when the air was exhilarating and brisk to the feelings. Altogether, the experiments were interesting rather than conclusive, and we may be allowed to express a hope that the committee will pursue their investigations still further as opportunities offer from time to time.

Other important communications and articles in the numbers of the journal under review are: "On the Temperature of the Sea between Scotland, Faroe, and Iceland," by the late Dr. Keith Johnston, and Mr. Buchan, the unwearying Secretary of the Society; "Notes of a Trip to Faroe," by Robert Tennent, Esq.; "Table of Observations connected with the Periodical Return of the Seasons" for 1870; "The Rainfall of Scotland," in a series of articles by Mr. Buchan; "Rainfall of the Day and Night at Smeaton, Haddingtonshire" (a new subject), from observations conducted by Sir Thomas Buchan, Hepburn, Bart., during twenty years; "On Underground Temperature," by the Secretary; "The Average Monthly Rainfall of Scotland," by the same; and "Observations Connected with the Periodical Return of the Seasons" for 1871. To each number, in addition, are appended the usual meteorological returns from the Society's stations.

In conclusion, it is with much pleasure that we chronicle the steady advance of this working Association. In January last the stations reporting numbered 105—94 being situated in Scotland, 4 in England, 2 in Iceland, 1 in Faroe, and 4 in foreign countries. There were 558 ordinary members at the same date; besides 9 honorary, and 13 corresponding members. All these figures testify to the increasing usefulness and popularity of the Scottish Meteorological Society.

VI.—Cholera: its Pathology and Treatment.[1]

In our last number having set forth in some detail the various modes in which cholera originates and spreads we now pro-

[1] 1. *A Treatise on Asiatic Cholera.* By C. Macnamara. London and Calcutta. 1870.

2. *On the Symptomatic Treatment of Cholera, with especial reference to the importance of the Intestinal Lesion.* By Dr. Felix von Niemeyer. Translated from the German by P. W. Latham, M.D. Cambridge and London. 1872.

3. *Lectures on the Principles and Practice of Physic.* By Sir Thomas Watson, Bart., M.D., F.R.S. Fifth edition. London. 1871.

pose to discuss the pathology and treatment of the disease. There are few diseases which have given rise to so many conflicting theories, theories obviously inconsistent not only with each other but also with the facts which they profess to explain. It may not be uninteresting or uninstructive to expound and to criticise some of these incomplete and erroneous theories.

One of the most remarkable attempts to explain the phenomena of cholera is that of Mr. Charles Macnamara. He believes that the disease is communicated by the agency of a specific poison which is contained in the choleraic secretions discharged from the alimentary canal. He assumes that this poison is swallowed, usually with contaminated water, and is thus brought into contact with the mucous membrane of the alimentary canal. Arrived there it does not become absorbed, so as to enter the circulation, but by a local action upon the mucous membrane it destroys the epithelium, more especially that of the intestinal villi. "The epithelium being thus destroyed, the serum of the blood is allowed to drain away from the capillary arteries of the intestines, and at the same time venous absorption is prevented from taking place through the mucous membrane. Hence the symptoms of cholera."

This theory is at variance with some of the most unquestionable facts of the disease. It is based upon an assumed anatomical condition, the destruction of the intestinal epithelium. Now, it has been shown by very careful and competent observers—by Drs. Parkes and Gairdner, some years since ('Medical Times and Gazette,' August and September, 1866), and more recently by Dr. Douglas Cunningham ('Seventh Annual Report of the Sanitary Commission with the Government of India,' Calcutta, 1871), and by Dr. F. R. Lewis ('A Report on the Microscopic Objects found in Cholera Evacuations,' Calcutta, 1870), that the shedding of the epithelium from the intestinal mucous membrane is the result rather of maceration in the alkaline contents of the bowel after death, than of an essential pathological change

4. *Epidemic Cholera.* By EDWARD AMBROSE FITZGERALD, Surgeon, Bengal Medical Service. Pamphlet. London. 1871.

5. *Notes on Cholera.* By GEORGE JOHNSON, M.D., F.R.S. London. 1866.

6. *Cholera and Choleraic Diarrhœa, their Nature, Cause, and Treatment.* By GEORGE JOHNSON, M.D., F.R.S. Pamphlet. London. 1870.

7. *Researches into the Pathology and Treatment of the Asiatic or Algide Cholera.* By E. A. PARKES, M.D., F.R.S. London. 1847.

8. *Symptoms and Treatment of Malignant Diarrhœa, better known by the name of Asiatic Cholera.* By WILLIAM MARSDEN, M.D. Fourth edition. Edited by ALEX. MARSDEN, M.D. London. 1871.

9. *On the Treatment of Cholera and Epidemic Diarrhœa, with a record of Cases.* By J. WILSON M'CLOY, M.D., and ROBERT ROBERTSON, M.D. 'Medico-Chir. Transactions,' vol. 50.

occurring during life. The destruction of the epithelium appears, therefore, to be an erroneous assumption, and there seems as little reason to believe that the escape of blood-serum would result from the removal of the epithelial covering of the intestinal villi, as that the symptoms of cholera are mainly and essentially caused by the so-called "serous discharges." The discharges, be it remembered, are not serous or highly albuminous but watery, with but a very small proportion of animal matter suspended or in solution; and we imagine that few pathologists at the present day accept the doctrine that the collapse of cholera is a result of the discharges from the alimentary canal.

Mr. Macnamara's theory is nearly identical with that propounded some years since by Niemeyer, who maintains that the characteristic symptoms of cholera "may without difficulty be referred to the severe and extensive mischief in the intestinal mucous membrane, and to the copious transudation from the intestinal capillaries." He, like Macnamara, denies the existence of a blood poison in cholera, he believes that during life the epithelium of the intestinal villi is thrown off and destroyed; he compares the state of the intestines in cholera to a portion of the external skin from which the epidermis has been removed by a blister, or by scalding, and he adds that "if we consider how great an extent has been denuded, we are at a loss to understand that any observer should speak of a disproportion between the anatomical changes in the intestine and the severe symptoms of the disease during life." In addition to what has already been said with reference to the removal of the epithelium from the villi being, in all probability, the result of post-mortem physical changes, we would ask the supporters of Niemeyer's theory by what means a poison, which sometimes enters the system by the lungs, can reach the mucous membrane of the intestines, except through the blood? Then, surely, it is obvious to all who are familiar with the history of cholera that the characteristic symptoms of the disease often reach their height, and again subside with a rapidity which is quite inexplicable by the hypothesis of extensive structural changes in the mucous membrane of the alimentary canal being the essential cause of the symptoms.

Again, we venture to suggest that Niemeyer's belief in the efficacy of calomel is scarcely consistent with his pathological views. Surely the administration of from two to four grains of calomel every half hour, or every hour, is little calculated to repair the assumed disorganisation of the intestinal mucous membrane. Yet he remarks upon this treatment that—

"After having vainly striven by means of opium and other remedies to check the constantly increasing choleraic diarrhœa, the motions becoming more and more serous and colourless, a few doses

of calomel were often effectual in removing it, so that we were compelled to credit the remedy we employed with the result produced."

Niemeyer explains the thickening of the blood in cholera and many of the symptoms of collapse by the discharge of liquid through the alimentary canal; yet he admits that a sudden arrest of the discharges during the stage of collapse is a sign of most unfavourable import. So fatal a condition is this known to be that he dreads nothing so much during the stage of asphyxia as the cessation of the vomiting and purging. It must be evident to all who have profoundly studied the phenomena of cholera that the attempt to explain the pathology of the disease, by reference solely to the abdominal symptoms, has been an utter and hopeless failure. Yet these symptoms in the great majority of cases form so constant and so prominent a feature of the malady that it requires more than ordinary care and caution to avoid being misled by their obtrusiveness.

There is one invariable condition common to all forms of collapse, and that is a failure of the circulation; but a failure of the circulation may result from very different physiological states. Failure of the circulation and consequent collapse may be a result of loss of blood or of blood constituents—from hæmorrhage or from excessive purging—it may be a direct result of an enfeebled action of the heart—from nervous shock or the influence of such depressing poisons as tobacco and antimony; lastly, the circulation may fail in consequence of some mechanical impediment to the flow of blood through the vessels. Now it is an unquestionable fact that a mechanical hindrance to the flow of blood through the pulmonary artery, the result of a fibrinous plug, suffices to induce all the most striking and characteristic symptoms of choleraic collapse. There are on record numerous case of embolism of the pulmonary artery in which the symptoms have borne a striking resemblance to those of cholera. The most completely recorded case of the kind is one published by Dr. Alfred Carpenter. ('Lancet,' Sept. 23, 1871.) In that case Dr. Carpenter remarks that "the only symptoms wanting to make it apparently one of cholera were alvine discharges and cramps in the limbs." The symptoms actually noted were blueness of the surface, coldness of the uncovered parts of the body, cold clammy perspiration, coldness of the breath, sinking of the eyes, feebleness of the voice, a feeble thready pulse with hurried breathing, excessive thirst and almost complete suppression of urine, two ounces of urine only being passed one day, and on another day less than two ounces. Here, then, we have a carefully observed and recorded case in which all the most characteristic symptoms of choleraic collapse were shown by a post-mortem examination to

have resulted from a plug of fibrin partially obstructing the pulmonary artery, and thus diminishing to an extreme degree the circulation through the systemic vessels.

A careful analysis of the phenomena of cholera affords evidence, scarcely less than demonstrative, that during the stage of collapse there is some mechanical impediment to the passage of the blood through the lungs. In no other way can we explain the fact that, while the extreme feebleness or even the entire absence of the radial pulse indicates a comparatively empty condition of the systemic arteries, the general lividity of the cutaneous surface shows that the systemic veins are full and even distended. Then the post-mortem appearances are in entire harmony with this view. When the disease has been fatal, during the stage of collapse, an examination made soon after death shows that while the left side of the heart is empty, or nearly so, the right cavities, the pulmonary artery, and the large systemic veins are distended. The lungs are anæmic, dry and light in weight, their colour sometimes pale, sometimes dark; the dark hue in some cases being a result of passive engorgement of the *bronchial* veins and capillaries, in common with the entire systemic venous system. When collapse has been very prolonged the lungs may become œdematous in consequence of a gradual serous exudation from the passively engorged bronchial vessels. The small specks of hæmorrhage which are commonly seen beneath the pleura and the pericardium are analogous results of engorged systemic veins and capillaries. Similar hæmorrhagic spots often result from the obstructed circulation caused by valvular disease of the heart, and these again are analogous to the ecchymoses beneath the skin and conjunctiva, which not unfrequently occur during an epileptic fit. The facts to which we have referred point with unmistakable clearness to some mechanical impediment in the course of the circulation during the stage of collapse. What then is the nature of this impediment?

A probable explanation of the phenomena is given in the following extract from Sir Thomas Watson's Lectures (vol. ii, p. 595). He says:

"Remember the abrupt contrast seen upon early examination of the body after death during collapse, between the anæmic condition of the lungs, and the gorged condition of the trunk of the pulmonary artery, and of the systemic veins. What is the explanation of this sudden arrest of the stream of blood in the small arteries just before it reached the capillaries? Were the arrest of motion due to gradual thickening in consequence of the continued abstraction of its liquid portion; it would be found stagnating in the capillaries as well as in the arteries. Bear in mind that one characteristic symptom of cholera — that symptom which irrespectively of the

fatality of the disease, renders it truly a disease to be dreaded—consists in very painful cramps of the larger muscles of the body. These contractions, it may be assumed, are produced by the choleraic poison, just as we know they are producible by the poison of strychnia. Dr. Johnson supposes that a similar cramped state of the muscular fibres which embrace the minute pulmonary arteries, is caused by the same choleraic poison, and bars these slender channels against the advancing blood; that the stop-cock action which I have so often explained to you comes here into play. The thickening of the blood is a consequence and not a cause of the arrested circulation and the collapse. Precisely the same blood thickening occurs as a result of the impeded circulation through the lungs which is associated with long-continued, extreme and fatal apnœa, as I have explained to you in a former lecture. The true explanation of the fact that mere diarrhœa however profuse, does not thicken the blood, is probably as Dr. Johnson suggests, that water is rapidly absorbed from the soft tissues to take the place of that which escapes from the alimentary canal. Acting on this principle of physiological hydraulics, we remove a dropsical accumulation by the action of a hydragogue purgative."

Sir Thomas Watson goes on to show that this theory of cholera "derives strong confirmation from the fact that it unlocks, like the true key, the whole of the pathological intricacies of the disease." It explains all the phenomena connected with the failure of the circulation, the defective aeration of the blood, the consequent lowering of the temperature, the small amount of carbonic acid thrown off by the lungs, and the nearly complete suppression of those secretions—bile and urine—whose constituents are products of oxidation, while the secretion of milk, in the case of a nursing mother, passing into the collapse stage of cholera continues apparently unchecked, and the breasts become painfully distended. The probable explanation of this curious fact being that the milk constituents—caseine, sugar, oil, and water—are unoxidised products, and may, therefore, be obtained from unaërated blood.

It has been suggested that the continuance of the mammary secretion during collapse is the result of a supposed antagonism between the nutritive and reproductive functions, it being assumed that the reproductive functions are unaffected by cholera. This hypothesis is quite inconsistent with the notorious fact that the death of the fœtus in utero, and the occurrence of abortion are frequently results of cholera. It is manifest, therefore, that the reproductive functions, as such, enjoy no immunity from the ravages of cholera.

The most probable explanation of the blood thickening in cholera is that which refers it to defective oxidation and partial stasis within the vessels.

Mr. Fitzgerald, who has seen much of cholera in India, referring to this question, says—

"There are other diseases, such as diarrhœa and dysentery, in which, though the discharges from the intestinal canal are often very profuse, we never meet with the same state of blood as in collapse. If, then, in ague, croup, and diphtheria, we have a dark, thickened state of the blood without any discharge of fluid from the body, and in diarrhœa and dysentery, where the discharges are frequently excessive, we never witness such a condition, surely it is not merely illogical, but actually perverse, to persist in maintaining that the thickening of the blood in choleraic collapse is mainly due to the drain of fluid from the body, and, in the face of sound argument and a powerful array of facts, to deny that it may result from some other cause."

If, then, the discharges from the alimentary canal are the essential cause neither of the blood thickening, nor of the collapse of cholera, what is their real significance and in what relation do they stand to the other symptoms of the disease?

The theory which appears to be most in accordance with the facts is thus briefly expounded by Sir Thomas Watson. It is held, he says—

"That the phenomena of cholera result from the entrance of a peculiar poison into the blood, where it probably undergoes, like that of smallpox, a rapid process of self-multiplication, and spoils certain of the blood constituents, which are then ejected through the mucous membrane of the alimentary canal; that the feelings of general oppression and malaise sometimes experienced before the onset of the bowel symptoms, are indicative of blood-poisoning; that the copious discharges are expressive of the efforts of nature to throw off a noxious material, and really form therefore a necessary part of the process of recovery; and that if the pouring forth of the vascular excretion be checked (as it can, perhaps, be by opium) the risk of fatal collapse is thereby increased."

It would appear, then, that the choleraic discharges are analogous to the cutaneous eruption of the acute exanthemata, but this theory by no means implies that the discharges are not a source of exhaustion, or that they may not be so copious as to destroy life. In like manner death may result from the abundance of the variolous eruption. So long as the circulation remains free the discharges bear a direct relation to the severity of the disease, but when with a partial arrest of the circulation through the lungs a state of collapse is induced, there ceases to be a direct relation between the discharges and the gravity of the symptoms, and in the worst class of cases there is an inverse relation between the discharges and the degree of collapse, so that in the words of Dr. Parkes—

"It may be confidently asserted that there is no one who has seen much of cholera who does not know that, exclusive of the mildest

forms of the disease, a case with little vomiting or purging is more malignant and more rapidly fatal than one in which these are prominent symptoms."

Mr. Fitzgerald says (p. 30)—

"In all the collapse cases that proved fatal in the Jounpore jail during the outbreak of 1870, vomiting and purging had invariably ceased, without the pulse being restored; and the sooner, under such circumstances, the vomiting and purging ceased, the more rapidly did the cases proceed to a fatal issue."

He adds—

"At first I had some difficulty in persuading the native doctors and hospital attendants that the vomiting and purging in cholera were not only not dangerous but actually good for the patient, and that the real source of apprehension was from failure of the circulation and suppression of urine. When, however, they saw all those cases turn out badly in which the vomiting and purging ceased without return of the pulse and urine, they grew more reconciled to this doctrine."

With reference to this question we extract the following from the very able and interesting paper by Drs. M'Cloy and Robertson, in the fiftieth volume of the 'Medico-Chirurgical Transactions:'—

"There is we believe no case on record of a patient having recovered from an attack of cholera without having experienced more or less the peculiar discharges of that disease. Upon looking over our record this will be seen to characterise every case of recovery. Whatever the treatment adopted the result was the same—*recovery never occurred without the continuance of the intestinal discharges or their restoration if previously arrested.* But if the collapse of cholera be produced by the drain of fluid from the blood consequent upon these discharges, surely recovery ought to be preceded by their *arrest*, and not by their continuance? Fatal collapse, on the other hand, invariably followed when the discharges could not be restored. May we not then reasonably look upon the discharges as salutary—as the agents by which nature frees the system from a deadly poison? We believe so, and with this belief we can no longer retain that hypothesis which demands their arrest."

Now, it is obvious that, if this be the true interpretation of the intestinal discharges and the collapse of cholera, the cure of the disease cannot be effected by means calculated directly to repress the discharges. "It is plain," says Sir Thomas Watson, "that if 'elimination' be a condition of recovery the method of elimination is nature's method, which art may help or hinder —help by the cleansing method, hinder by the astringent." We have reason to believe that this principle of treatment is now accepted and acted on by a very large proportion of the

profession, but it has had to make its way in spite of much opposition and prejudice, and it is manifest that much of the opposition has been based upon a misapprehension. Most writers who have expressed their dissent from the eliminative treatment of cholera erroneously assume that the object of those who advocate this method of treatment is to *increase* the discharges from the mucous membrane of the alimentary canal, and they proceed to argue that, if this is the correct principle of treatment for cholera, its application to the treatment of other diseases would result in attempts to increase the eruption of small-pox and the other exanthemata. Dr. Bristowe, in his "Croonian Lectures" ('Brit. Med. Journal, June 1, 1872) adopts this line of argument, and asks whether—

"Any one ever attempted to eliminate mumps by promoting saliva or semen, or diphtheria by encouraging the development of its characteristic false membrane? And yet, if the theory of elimination be true, I conceive that this should be the proper indication for treatment."

This course of reasoning is based upon an entire misconception of the object proposed by the advocates of the eliminative treatment of cholera, which is not to increase the discharges from the mucous membrane, but simply to prevent their retention and to facilitate their expulsion from the alimentary canal.

Upon this point Sir Thomas Watson remarks that—

"Whatever may have been Dr. Johnson's earlier purpose, he does not now propose to *excite* discharges from the mucous surface of the digestive canal; but simply to facilitate the removal of matters lodged there;"

and Dr. Johnson himself says—

"The object of a purgative in cholera is not to increase excretion from the blood into the stomach and bowels, but simply to assist in the expulsion of the morbid secretions from the digestive canal." ('Notes on Cholera,' p. 89.)

He also expresses his belief that copious draughts of cold water will suffice for the cure of most curable cases of cholera, and he maintains that this cleansing mode of treating cholera is strictly analogous to the treatment of small-pox by free ventilation; the object in both diseases being to facilitate the removal of noxious secretions which have been spontaneously ejected from the circulation. The attempt to excite prejudice against the eliminative treatment of cholera by comparing it with the old heating method of treating small-pox appears to us singularly ill-judged. The treatment of small-pox by excess of clothing, and by closed doors and windows, was the reverse of eliminative; it was a pernicious and fatal practice, not because it increased the

cutaneous eruption, but because it compelled the unhappy patient to breathe a close and poisonous atmosphere, and so it was in reality more analogous to the astringent treatment of cholera than to the treatment of that disease by cold water and evacuants.

It is not our intention now to discuss the details of treatment. There is a very general agreement that during the stage of collapse opiates and astringents are injurious, and diminish the probability of recovery. It is also acknowledged by those who are most capable of forming a judgment that, in cases of extreme collapse, none of the ordinary means of treatment have any perceptible influence upon the progress of the malady. If the proximate cause of choleraic collapse be, as we have reason to believe, an arrest of the flow of blood through the lungs, it is obvious that ordinary remedies are powerless to overcome that obstruction, while the immediate and marvellous, though too often temporary relief afforded by hot saline injections into the veins affords additional evidence of the mechanical nature of the obstruction upon which the state of collapse essentially depends. Assuming, as there appears reason for doing, that the morbid blood excites the contraction of the minute pulmonary arteries, it is probable that the beneficial influence of the saline injections may be explained partly by the fluid diluting the blood in the right side of the heart and in the pulmonary artery, and so rendering it less stimulating to the tissues with which it is brought into contact, partly by the relaxing effect of the warm liquid upon the arterial spasm. It has been conclusively proved that merely tepid injections are followed by much less striking results than the injection of fluids of higher temperature. The late Dr. Mackintosh injected no fewer than 156 patients during the epidemic of 1832. The temperature of the fluid injected by him varied from 106° to 120°, and he states that "the good effects of the injection were rapid in proportion to the heat of the solution." (Mackintosh's 'Practice of Physic,' vol. i, p. 365.)

Mr. Little, during the last epidemic, injected at the London Hospital twenty patients who were considered to be in hopeless collapse, yet six of these patients recovered. The temperature of the saline fluid injected was 110°, and Mr. Little states that "a temperature below 100° is decidedly injurious." ('Medical Times and Gazette,' April 6th, 1867.) On the other hand Dr. Parkes obtained much less striking results from injections of lower temperature—98° in one case, and in another said to be "tepid," but the actual temperature not mentioned (p. 219). The effect of a warm bath in relieving the muscular cramps is very striking, and the radial pulse not unfrequently increases in

volume and power while the patient is immersed in the bath. These facts are confirmatory of the proposition that a high temperature tends to relax spasm, both of voluntary and involuntary muscular fibre.

While the immediate and most striking results of saline injections into the veins are probably due to the relaxation of the arterial spasm; it is not denied that the injection may indirectly assist recovery by increasing the liquid medium for conveying the poisonous products out of the vessels, just as a copious imbibition of water helps to wash out and cleanse the bowel.

In the diarrhœa stage of cholera, as in cases of ordinary diarrhœa, that plan of treatment is obviously the best which most speedily and completely arrests the disease without subsequent ill effects. We believe that in the majority of cases no active remedies are needed either of an evacuant or an astringent nature, and that simple diluents, by favouring the natural tendency to recovery, will usually suffice for the cure. An indiscriminate opiate and astringent treatment of diarrhœa is now adopted by comparatively few practitioners, while, on the other hand, the practice of commencing the treatment by a laxative dose is very general. In the words of Sir Thomas Watson:

"No doubt the true indication of treatment is to stop the flux as soon as possible; but this may sometimes best be effected by (as also in crapulous diarrhœa, and in the summer cholera of Sydenham) by carrying off the offending matter."

The true use of evacuants in this class of cases is to facilitate and quicken the expulsion of undigested food and morbid secretions, and to prevent painful distension of the bowels; while, on the other hand, the true use of opiates and astringents is to soothe the bowel, and to prevent excessive secretion after the expulsion of their morbid and irritating contents. Tact and experience alone can teach the practical use of these remedies in each case.

In the treatment of cholera, in all its stages, we strongly deprecate the employment of needlessly active and dangerous remedies. We have repeatedly referred to Mr. Fitzgerald's able pamphlet which contains much interesting matter relating to the etiology and the pathology of cholera, but we are surprised to find that he considers strychnia to have an almost specific curative influence upon the disease, and he advises the employment of that powerful drug in, what we consider, dangerous doses. He gave one-twelfth of a grain every twenty minutes until half a grain was taken, then an equal dose at longer intervals. Some patients had as much as a grain and a half in the four and twenty hours. It is probable, as he says,

that "much of it may have passed away with the dejecta without being absorbed, and that this alone prevented the toxic effects of the drug." He proposes in future to administer strychnia hypodermically, and he considers that an eighth of a grain may thus be given as a first dose. Now, considering the probability that in consequence of the drain of water through the alimentary canal there would be an active absorption of liquid from the subcutaneous tissue, we consider this a dangerous dose of strychnia to be thus administered; and, so far as we can learn, there is no evidence that this poisonous drug has a curative influence in any stage of cholera.

The same objection applies, although in a less degree, to the large doses of calomel recommended by the late Dr. Marsden. We find that Dr. Alex. Marsden, in the preface to the new edition of his father's little book, expresses his opinion that since the first publication of that work no advance has been made in the scientific treatment of cholera.

In this statement we think that he scarcely does justice to his contemporaries. In the original work it is stated that "infectious diseases never attack a human being a second time," and cholera is classed, as a disease which is neither infectious nor contagious, with plague and typhus fever.

As this statement is allowed to remain without editorial comment it may be taken as the expression of Dr. Alex. Marsden's opinion upon contagion in general, and upon the contagiousness of plague, cholera, and typhus in particular. We scarcely need add that this doctrine is not in accordance with the conclusions of modern medical science.

VII.—State and Progress of Therapeutics.

The science and art of therapeutics now assume a leading place in medical thought and study. It is, indeed, only natural that the most prominent function of medical men—that which they have to do day by day, and which is involved in the most limited exercise of their craft, should occupy a prominent position in their thought and training; yet it is only just now that therapeutics have acquired such a position.

Our relation in regard to the subject is this:—we do know a great deal, but we are continually obliged to act as if we knew much more. We cannot stop our art, even if we would, until the science thereof becomes perfect.

What concerns us is to show that therapeutical science rests upon a firm and impregnable basis. How we cure, we often

know not; but it can be proved that we *do* cure, and do so sometimes very immediately and efficiently. If we are to wait until we understand the *rationale* of all our remedies before they are used, we shall have to stand idly by and see death win many battles that might have been won by ourselves. This may be empiricism, but it is an empiricism which governs our conduct in many other callings of life; nor can we ever completely escape it, so long as our capacities are what they are.

We have suffered much from the darkness and the mystery which have shrouded the history of particular medicines; while tradition has added its obscurity and its conventionalities. There are certain medicines whose healing properties are suggested by their physiological action, so that the former can be accurately inferred from the latter; but a number of drugs must be termed empirical, in the logical sense that the laws of their action are empirical. Their effect on the healthy human system tells nothing how they will behave towards disease—whether they will prevent it, modify it, or even add to it. There are particular medicines which act towards particular diseases with as much certainty as one physical law is the antecedent of another; the disease and the remedy are bound together by the strongest ties of material association. Take colchicum as an example. Nothing in the natural or chemical history of colchicum informs us that it may help to subdue a paroxysm of acute gout, usually with safety and speed; but directly this fact is established in our personal clinical experience, the tendency is to draw large and vague conclusions, and to imagine that colchicum will cure or relieve other diseases *like* gout. And so we prescribe colchicum in a case of rheumatism, for instance, for no other reason than because rheumatism resembles gout in some of its objective symptoms. No one has ever proved that colchicum does any good to rheumatism, it is a mere hasty inference rushed at, or hoped for, partly because we have no better weapon at hand. On such weak premisses we sometimes build. Conventionality bids us to do this or to do that, not from any abiding logical faith, but simply because some one has done this or that before us; authority and reason may go together, but often authority over-rides reason, and defies her experience and her teaching.[1]

[1] Iodide of potassium, almost the *facilè princeps* of medicines, is prescribed very much in the same conventional way. It relieves some forms of gout and rheumatism, and *therefore* it is administered with the hope of controlling rheumatoid arthritis, although it never does any good whatever to this disease. But we do not wish altogether to discourage random prescribing. Obscure nerve-lesions, paralysing and eventually killing, may be entirely abolished by the aid of iodide of potassium; and even when there is not the remotest evidence of syphilis, it may

Untold discredit is brought upon medical art by ignorant routine; and that routine is most hurtful which is pursued under the outward badges of diplomas and degrees. For a more perfect illustration of the strong influence of traditional authority, we select two articles of our creed, the one surgical, the other medical; and we propose now to examine the belief and the practice respecting each of these articles, and show how far they may be said to rest upon the solid basis of scientific truth.

We will discuss first the surgical doctrine of *bloodletting*. The battle-ground about bloodletting has been mainly this:—Does it cut short disease (*i. e.*, the pathological processes which make up disease), or does it only relieve symptoms? In 1858 and 1859 Dr. Hughes Bennett addressed himself to this problem,[1] and with his usual ability maintained the thesis that disease, as such, is not cut short by letting blood; that, on the contrary, the real disorder is prolonged, and rendered proportionately more fatal by that practice. On the other hand, he contended that small bleedings, which do not lower the vital strength, are sufficient to relieve urgent symptoms; it being recognised that the lesion otherwise is to be assisted in its natural progress. Dr. Hughes Bennett was an unqualified opponent of the change-of-type theory—a theory which he says was invented to reconcile the apparent discrepancy between the practice of past and present days: he argues that more knowledge has suggested a better practice, and one more in accordance with sound principles.

Dr. Markham's papers,[2] published about the same time, attracted notice because of the clear distinction with which he drew between the two classes of cases—those in which venesection does, and those in which it does not, act beneficially in inflammation. He shows that bleeding is useless, *quoad* the inflammation; but that it is frequently of service by relieving certain of the secondary consequences which result from the inflammation, as, for instance, congestion of the heart. He alleges that bleeding is never beneficial except when this congestion of the heart exists: and he quotes cases of diseased conditions of various viscera, in all of which cardiac congestion secondarily arose, and in which bleeding caused relief. These views are developed at length in the "Gulstonian Lectures for 1864," as well as in a clinical lecture delivered by the same author soon afterwards.[3]

be well to make a chance hit on the bare possibility of realising a magnificent success.

[1] 'Principles and Practice of Medicine,' 3rd edit., pp. 297—311.

[2] 'Lancet' and 'Edin. Med. Journ.,' 1857 and 1858.

[3] Reported in the 'Brit. Med. Journ.,' for April and May, 1864, and Feb. 4, 1865.

The doctrine thus propounded has the merit of great simplicity, and becomes intelligible even to the non-medical mind. It is resolved into a question of pure statics, and so is rendered amenable to ordinary static laws. In his intense energy against the old savage system of blood depletion, Dr. Todd looked at the subject from a too purely vital standpoint; within the circle of vital phenomena his argument was right, but he omitted to see that the circulation has a physical side to it, and that a merely mechanical derangement of the circulating fluid may be put right by the timely withdrawal of a portion of that fluid. Both in medicine and in theology, the doctrinal pendulum swings from one extreme to another, and so entirely did Dr. Todd's opinions govern medical practice for a few years, that Dr. Markham tells us, with an honest note of exclamation, that his house-surgeon had never bled a patient, nor had seen a lancet used in bleeding! We are sure that the house-surgeon learnt from his master a valuable lesson in hæmostatics; and we hope that many others, since that time, have had the subject rescued from the confusion which comes from trying to obey too literally the precepts of opposing teachers. The truth, as now enunciated, is so plain that there can be no excuse for misunderstanding it. We ought not to bleed in a pneumonia, for example, unless that pneumonia be so extensive as to seriously disturb the play of heart and lungs. When we are asking ourselves in any given case whether we are to draw blood, we should not think of the inflammation at all, but study the condition of those organs or parts which have become secondarily engorged and embarrassed as a consequence of that inflammation. Our desire should be to save sound organs from disease, and to keep them sound, and to restore freedom of action whenever and wherever it has been interfered with.

The wave of recoil from the non-bleeding epoch of the last quarter of a century has been ably and philosophically followed up by Dr. B. W. Richardson, who extends the practice laid down by Dr. Markham to kindred regions of pathology. Dr. Richardson advocates the letting of blood in cases of lightning-stroke, sun-stroke, and in some cases of reaction from exposure to cold; all these instances having the common ground of mechanical derangement of the circulation from shock to nerve-centres. Further, he proposes a return to the ancient plan in some forms of cardiac congestion, going hand-in-hand in this matter with Dr. Markham; and finally, in the more doubtful diseases called uræmic coma and apoplectic coma, Dr. Richardson adduces some forcible arguments in favour of the traditional practice, which, if it do not always cure, relieves sometimes in quite an unexpected manner.[1]

[1] See papers in the 'Practitioner,' Nov., 1868, and in the 'Med. Times and

Out of this abundant material for judicial summing-up, we are confident that the profession will gradually resort to more moderate and stable ways. Directly we act as a body from settled principle, there will no longer be among the public that flux and reflux of opinion which are the products of ignorance and fright. It is quite true that "a hundred years ago a surgeon or a physician who refused to bleed in apoplexy would have lost caste on all sides, and would probably have been considered guilty of malpraxis." It is equally true that a medical man who now opens a vein and lets flow only a few ounces of blood must be prepared for an overwhelming condemnation from a chorus of amateur voices, if the patient should happen to die soon after it, or suffer from any sort of prolonged weakness.

We might now review the long-vexed, the fluctuating and even contradictory opinions which have hovered around that important medicine, digitalis; but we shall simply recapitulate what has been established by the careful observations of Dr. Sydney Ringer[1] and Dr. Fothergill.[2] When the symptoms and physical signs point to great dilatation of the left ventricle, with a not inconsiderable amount of hypertrophy, digitalis will be found to be of eminent use. The doses of the medicine are always to be as small as possible, and the infusion is stated to be the best form of administering it.[3] The absence of valvular disease enhances the value of the digitalis; and the irregularity of the pulse is the most important indication for the medicine. Paroxysmal dyspnœa, dependent on palpitation of the heart, is alleviated by infusion of digitalis. The medicine does no good in dilatation and hypertrophy of the right heart, secondary to chronic bronchitis and emphysema of lungs; nor is its use demanded when the left ventricle of the heart is hypertrophied as a sequel of aortic valvular disease. It relieves palpitation, and this must be by a tonic action on the heart. Digitalis is useful chiefly in defective expulsive power of the heart-walls; under its use the contraction of the ventricles becomes complete, the pulse steadier and firmer, dyspnœa is abated, and urine is secreted more freely. In this manner dropsy may be indirectly removed. For persistent use Dr. Fothergill recommends that the drug should be given in powder.

These results seem very trustworthy, but they have been got

Gazette,' Dec. 17, 1870. The treatment of uræmic coma is more fully discussed in the 'Asclepiad.'

[1] 'Practitioner,' Jan., 1870, and 'Handbook of Therapeutics,' 2nd edit., pp. 324—342.

[2] Hastings' 'Prize Essay.' 1870.

[3] Dr. Anstie points out that the apparent results obtained when the tincture is used are quite indecisive, as the action is complicated by the influence of alcohol.

at only after many experiments and cross-questionings; unusual difficulties have been surmounted, and inductive logic has won the day. Of this laborious investigation we are now enjoying the fruits at our ease; and they appear so facile to apply, that we are apt to forget the trouble by which they were secured.

But other medicines need the same toil and thought to be spent upon them. Four or five years ago a sub-committee of the British Medical Association accomplished some excellent work on the physiological and therapeutical effects of mercury. Dr. John Harley's investigations on the "Old Vegetable Narcotics" deserve the heartiest praise; seldom out of Germany have there been more accuracy and industry. Not only have his researches been conducted in the best manner, but the important conclusion about Conium has been established, that "a dose which falls far short of producing its peculiar physiological effects is of no more use than an ordinary dose of quinine in the treatment of ague." Every one can see at a glance the far-reaching scope of this doctrine. Often do we miss our aim in trying to alleviate a disease by the aid of a particular remedy from fear lest we should accidentally and temporarily hurt the body by its action. There is good reason for seeking to lessen that hurt,[1] but no reason at all for abstaining to use the medicine which causes it. Drugs are sometimes not to be weighed in coarse material scales, but to be distributed according to the necessities of the sufferer. "Doses" are the most relative things in the world. It must be confessed that a certain maturity of mind and boldness of action are requisite to escape from the slavery of posological entities and essences, and to allow the apparent exigencies of the case before us to be our sole guide. That constitutional bashfulness which is called "caution," which habitually delights in small ways, and which is half-afraid of the instruments it uses, should practise other arts than the art of medicine. A wise courage is the physician's watchword.

It is satisfactory to the practitioner of medicine to find that his resources are increasing every day; but it is not less pleasing to the philosophic observer that the methods by which these resources are acquired are eminently trustworthy and safe. Bromide of Potassium was at one time a byword of uncertainty, no one knew exactly what good it could do, nor when it would do it; but we felt that there was a treasure of therapeutic goodness locked up in this medicine which it was our duty to discover and to formularize. Like other drugs belonging to the same group, Bromide of Potassium did not and could not tell

[1] As, for instance, by the plan proposed by Dr. Spender, of Bath, in his paper recently contributed to this Review, on the "Administration of Medicines in comparatively Small and Frequent Doses."

its own tale; we learnt nothing from its chemical composition or reaction, nothing from its crystallography; even a study of its physiological effects did not land us on any sure ground. And there was a danger of our confusion becoming greater when the bromide got mixed up with that "unhappy word," hysteria. But many of the useful secrets of the remedy are now disclosed, and probably more will be found; and, better still, what we have already learnt of it may be defined within limits easily recognised by the practised therapeutist.

Among the excellent pieces of workmanship for which we are indebted to our brethren in the United States, we may specify the physiological investigations of the two alkaloids of *veratrium viride*, by Dr. Horatio Wood.[1] And how are we to speak in sufficiently thankful terms of those blessings to the physician and the surgeon respectively—chloral and carbolic acid:[2] Innumerable is the army of those who have explored the uses of these remedies, and who can attest by personal experience their aid in the art of healing. The medical application of chloral marks an epoch almost as much as the discovery of chloroform. Calabar bean, too, is a drug of the highest value.

There are three auxiliary agents which have been for the first time placed on a comparatively clear and positive basis within the last few years. We understand a greal deal more than we did about the use of Alcohol in diet and disease. Many and fierce have been the combats on this question, and the extreme views which have been upheld represent equal divergencies from reason and knowledge. To the editor of the 'Practitioner' and his coadjutors we are under an obligation for an exposition of the dietetic and medicinal qualities of wines. Secondly, the therapeutic virtues of electricity have been most satisfactorily illustrated by various home and foreign *savans* (among Englishmen we can name Mr. J. Netten Radcliffe and Dr. Russell Reynolds); and we have in that agent one more sure weapon to combat the endless varieties of palsy and neuralgia. And thirdly, numerous travellers (scientific and amateur) have instructed us where to find mineral waters, what they are made of, when to drink them and how to bathe in them. "Fashion and fancy" have had an unusual sway in this matter. And Sir Henry Holland, in his interesting 'Recollections of Past Life,' reminds us of the

"Similar fashions of the Romans as to mineral waters, implied in

[1] 'American Journal of Medical Science,' lix, p. 36. Oulmont's researches on the same drug ('Bull. Gén. de Thérap.,' lxxiv, p. 145) deserve a passing record. The 'United States' Dispensatory' is a storehouse of therapeutical ideas which it would be extremely profitable for British practitioners to study.

[2] Dr. Sansom's enthusiasm on behalf of the medical use of the sulphocarbolates deserves commemoration here.

the sarcastic words of Pliny—*Medici qui diverticulis aquarum fallunt ægrotos.* It must be admitted that this phrase is applicable to the usages of our own time and country, as regards this part of medical practice. But the fault is not on one side only. The plea of health, often put in at the end of the London season, to obtain sanction for a watering-place abroad instead of a country house at home, is one which taxes both the conscience and the judgment of the physician."

One of the most promising aspects of materia medica is the increasing study of remedies from their chemical relation to the animal tissues. We shall not easily forget the impression made by a paper read at the Bristol meeting of the British Medical Association (1863) by Dr. C. B. Radcliffe, on the treatment of neuralgia.[1] The philosophy of the paper was so simple and so obviously true, that men asked themselves—surely "all this has been said before?" Well, if it had, certainly never with such homely point and illustration. Nerve-pain being assumed always to connote deterioration of nerve-tissue, we were invited to remember that the diet of the neuralgic patient should not contain too much meat and too little fatty or oily matter; to look upon the properly-regulated use of alcoholic drinks as essential to success in treatment; to give some preparations of phosphorus, with cod-liver oil, as nutrients for starved nerve structure; and, as a rule, to avoid the use of sedatives in sedative doses. As a bare theory, all this seemed very rational and conclusive; as a question of practice, we hardly know anything to be more thoroughly satisfied with than the nourishment of poor crying nerves by dietetic and medicinal fats. Tired of the mere stifling of pain for a time by quinine and hypodermic morphia, we yearn for a therapeutic *backbone* to our efforts, and like to feel that the foundation for a permanently curative work is being laid. In Dr. Anstie's exhaustive treatise on neuralgia, the importance of the administration of cod-liver oil is fully recognised, and we wonder that this point has been overlooked in one of our best therapeutic hand-books (Dr. Ringer's). Then, again, in many chronic diseases of the skin, there is the same clear chemico-dynamic reason for giving cod-liver oil and the fatty elements of food. Similarly, phosphorus has enjoyed a high repute in the form of the hypophosphites of potash and soda as a remedy for phthisis and neuralgia,[2] and also in its pure state for some inveterate diseases of the skin (Burgess, Broadbent, Erasmus Wilson).[3]

It may be said that the 'British Pharmacopœia' represents

[1] Reported in the 'Brit. Med. Journ.,' Nov. 7, 1863.

[2] Dr. Radcliffe, loc. cit.

[3] 'Journal of Cutaneous Medicine,' ii, p. 304. A paper by Dr. Broadbent is referred to, entitled "An Attempt to apply Chemical Principles in Explanation of the Action of Remedies."

the most complete dogmatic teaching of the profession. Successive editions embrace our advancing knowledge, and although flaws have been detected by the microscopic scrutiny of pharmaceutical experts, we may fairly be proud of what may be called our national museum of therapeutics. Scarcely any codex is so rich in resource, or so lavish in variety; and we need not travel out of its bounds even for the "sweetness and light" of pharmacy, as the preparations of chloroform cover almost any severity of nastiness and discomfort.[1] In this matter how often do our continental brethren proclaim their superiority? The stores of the French *pharmacien* vie with those of the pastrycook in their charming guise, and the apothecary sends out his wares in granules of concentrated and tasteless alkaloids. Dr. Burggraeve, of Ghent, announces to the European public the new doctrine of pleasant physic, and tells us of a number of active principles which are little known, but of every-day value.[2] We must risk the suggestion that these globules are not so stable in their composition as might be expected; our uncertain climate plays sad tricks with the essences and potentialities of vegetable medicines, and often reduces the pretty granule to the inertness of a sugarplum. Still, it has been justly said that the "notion that medicine ought to be strong and nasty has passed away," and we ought to be solicitous to allure our patients to their own good, lest by neglect a slight disorder becomes a serious disease. And nasty drugs have a great deal to answer for this procrastination.[3]

The practical question has now to be answered—What is the apparatus for educating our students in this wide system of medical therapeusis?

We try to call to mind what was done for us as students by the great educational powers who then governed the schools; what was learnt during the old system of medical apprenticeship; and we shall proceed to inquire what are the provisions

[1] The Faculty of Medicine at Paris, in the Preface to their 'Codex Medicamentarius or Pharmacopœia,' expressly disclaim any intention of hindering practitioners from using other remedies, or apothecaries from keeping other articles, besides what are mentioned by them; and further observe, that they have inserted several popular medicines, although not likely to be ordered by the faculty themselves, in order that they may be uniformly prepared, and, of course, uniform in their action.

[2] In his 'Guide de Médicine Dosimétrique Dr. Burggraeve specifies bryonine, caffeine, scillitine, cicutine, inuline, &c., &c., as of medicinal efficacy.

[3] An extinct and repulsive heresy shall have an epitaph here. Ten years ago a Dr. Hagers published a book on Isopathic Remedies (*Medicamenta isopathica*); by these the author meant the morbid products proper to combat the diseases which furnish them. We may judge of the nature of these remedial agents by the following specimens:—*Coryzicum*, the mucus of coryza; *herculicum*, the foam of an epileptic; *leucorrhicum*, leucorrhœal discharge!

now existing for sufficient instruction in the therapeutic art.

In bygone years young men were expected to cram themselves with amazing details about the botany and natural history of leaves, barks, woods and roots, with the geography of medical plants, with the chemistry and adulterations of the inorganic materia medica, and with a frightful quantity of facts relating to the pharmaceutical preparation of medicinal substances; but when the student developed into the practitioner, what had he of that ready knowledge about the power of medicines by which alone he can cure disease? If he were at once thrown into a busy practice, without an older mind to help him, he found that five cases out of six were strictly medical, requiring an instinctive familiarity with tools that no one had taught him properly to use. If he had painfully to work out his own line of practice, he had to learn his art from his own failures and shortcomings. It is really extraordinary that teachers and writers on materia medica did not remember the ordeal which they have themselves passed through, and did not gather therefrom the elements of knowledge requisite to secure professional success.

Emphatic and unanimous is the revolt against the old methods of teaching, and a protest has been eloquently recorded in the recent number of a medical periodical:

"We earnestly believe that the subjects of pharmaceutical chemistry and botany are as nothing, in real and practical importance, compared with that grasp of therapeutical principles which is the very salt of life to the scientific intelligence of the student of medicine, that we are determined to protest, on every occasion, against the routine system which tends to swamp that intelligence under a mass of almost totally useless learning, while things of essential importance to his progress are neglected. We do most unfeignedly and heartily agree with Professor Huxley in his expression of impatience at the well-known pedantry which, for generations, has assumed that the practitioner of medicine is bound to possess an accurate knowledge of matters which really belong solely to the pharmaceutical chemist. Now, less than ever, is there any excuse for the continuance of this system. Our pharmaceutists of the future will be thoroughly competent men, and it will be their interest, no less than their pleasure, to supply good and pure medicines to the profession and the public. The day for bulky treatises on materia medica (in the old sense) for the medical student are over. It is fully time that his energies should be left free for the serious prosecution of those studies in therapeutics, properly so called, which will give him a sense of reality in his work that has been sadly lacking."[1]

[1] 'Practitioner,' April, 1872.

The ideal physician of the future, then, is presumed to be supplied with fitting materials for the exercise of his craft; nor need he (as a rule) trouble himself where they come from, nor how they are prepared. The responsibility of *right use* lies exclusively with the practitioner, and, therefore, it is of supreme importance for him to be properly taught. The 'British Pharmacopœia' should be the text-book of the teacher, and out of its abundant resources he should select some common and powerful substances for comment and illustration. How really rich the student would be who leaves his school with a fair knowledge of the medical qualities of mercury, opium, iron, and iodide of potassium! And this knowledge practical experience would extend and ripen every day. Clinical lectures might be given, not on pathology and diagnosis, but on the efficacy of special drugs, on their applicability to particular diseases, and the degree to which one medicine is interchangeable with another of the same kind. And an immense amount of collateral information would be conveyed by the discussion of some important disease like syphilis, and a clear description of the stages at which mercury or iodide of potassium would be necessary, with the quantity and frequency of administration.

Keeping still before us three or four important medicines (and the *status pupilaris* need not be perplexed with more than about one third of the 'Pharmacopœia), the next grade of instruction should consist in an exposition of the officinal preparations of mercury, iron and opium respectively. Making a metaphor out of grammar, a drug has its moods and tenses just as a verb has; the grammarian knows the correctness and propriety of every such mood and tense, and the therapeutist selects with no less precision the form which will best execute his purpose. And this knowledge can be best imparted at the bedside.

Is the art of prescribing so easy that it needs no teaching? Do balance, symmetry, and form come by accident? A well-written prescription is a highly-finished product of tact and knowledge; it cannot be forged by a mere quackish pretension to knowledge; it is a symbol of delicate and discriminative judgment of diseases and their remedies. Every prescription ought to be compact and harmonious in a therepeutic sense, and compatible in its chemical and pharmaceutical qualities. Can this standard be approached, even a long way off, unless mental habit and training be steadily directed to it?

It is within our personal experience that "qualified" men often begin practice who are gifted with a large store of clinical learning, but who are utterly unable to write a decent prescription. Well, this difficulty may be soon overcome; but other

dangers are not far away. It is exceedingly easy to travel round and round within a very narrow circle—to become a "routinist" in a bad sense of the word—and that not because there are not plenty of materials within reach, but because it is too much trouble to charge the memory with them and to apply them. This is a downfall to a state of laziness seldom recovered from. The fresh and early years, when a man may be a student in the highest meaning of the word, should be employed in gathering from all quarters the works of the "old masters;" in studying the infinite combinations of remedies, and the various subtle ways by which we may compass a specific end. Is not all this as worthy of consideration as the shape of a splint, the material of a ligature, or the curve of an obstetric forceps? The properties of all the formulæ of opium and its derivatives, and their uses under various circumstances, the splendid efficacy and special dangers of hypodermic morphia; and the emergencies which can be appeased by opium or other narcotics respectively, form in the aggregate a subject deserving of patient thought and elucidation, It was Prometheus who

> ——— "told the hidden power of herbs and springs,
> And disease drank and slept."[1]

No gold can buy a knowledge of this "hidden power," but more and more of it can be learnt every day by the honest searcher after therapeutic laws. It is vain to sigh now for the ancient days of medical "apprenticeship," but many fruitful seeds were sown in those days; and he was an idle pupil who did not reap many a good thing from the hands of an apt teacher, to pass in time to others the sound traditions of our art.

We have not Dr. Paris's "Pharmacologia" by our side, but we well remember the rules and ordinances laid down there for the interior structure of prescriptions. There is to be a regular procession of substances, according to gradation of rank. The most important article ought to be named first, then the article next in power; auxiliary tinctures and syrups come in due file and order; and the aqueous menstruum brings up the rear. What a torture it must be to the ghosts of old classical physicians to see the "directions" written in plain vulgar English, ostensibly to avoid mistakes with the dispenser, but too often in reality to hide the prescriber's ignorance of Latin phrase and Latin idiom. And, oh, behold the countless species of the genus prescription! There is the bald prescription, consisting of a single substance dissolved in water, without any ornament of syrup or tincture. At the other extreme is the rhetorical and

[1] Shelley's 'Prometheus Unbound.'

florid prescription, made up of eight or ten articles, most of which are themselves compounded of several others. Between these points comes the prescription of meek and conventional stamp, not rarely the sure index of poor and penurious resource; alas! how often met with here and everywhere. Then there is no more certain sign of equal mind and genuine accomplishments than a prescription of emphasis and nerve, telling its own story of single aim and object. We are grateful to Mr. Beasley for collecting so much lore; but we wish that he had weeded his garden, for the flowers are nearly choked. What benefit is it to know the things prescribed by Wendt, Recamier, Tode, and Foy; their utterances may furnish a curiosity shop, but otherwise they are of little value. If Mr. Beasley had bravely discarded 2000 of his prescriptions, and carefully classified the remaining thousand, the boon he has offered to us would have been more artistic and more truly accessible. We want, says Addison, less of "show and ceremony, and more natural good sense and beauty."[1]

The wide interest displayed in every therapeutic discovery is a guarantee for the spread of therapeutic knowledge among all ranks of the profession. When we call to mind the slow and sceptical way in which cod-liver oil was received, we are cheered and encouraged by the history of chloral, and the almost joyful rapture with which it was taken up, as if it filled a broad gap in our defences. The student leaves his school with a more intelligent faith, and better equipped for the exigencies before him. He may have been among the audience of such a course of lectures as Dr. Brunton's on "Experimental Therapeutics;" and though he has caught only the faintest echoes of the lecturer's meaning, he is impressed with the method by which, out of frail dynamic elements, positive knowledge has been gained. Scoffs at the uncertainty of physic are no longer heard from professors' chairs; but learned men pleasantly debate whether Nature is a healer of our troubles by "elimination," or whether she is a cruel "stepmother," destroying more bodies than she saves. Awkward hypotheses about "change of type" in disease are hardly necessary now; the problem is solved by a mere glance at our progress, both in our means of diagnosis and in our enlarged powers of treatment. If any one desires to be acquainted with what has been done in a single subject let him peruse the "Lumleian Lectures" for 1843, delivered by a distinguished living physician; the third lecture is on "Affections of the Brain and Spinal Cord depending on Acute Disease of the Heart."[2] In the refinement of our instruments for

[1] 'Spectator,' 119.

[2] 'London Medical Gazette,' May 25, 1843.

detecting morbid changes and their pathological relationship with other changes, do we not discover a *vera causa* for the doctrines of "support" and "rest," instead of starvation and depletion? Pericarditis and endocarditis must be the same now as then, and so must be the disorders which they provoke and resemble. And thus, having boundless faith in the certainties of medicine, the practitioner will give advice to his clients with (as Dr. C. J. B. Williams says) "confidence and clearness," making them willing sharers of his own faith. And though there may be mystery in what we do there is no miracle. The mystery decreases day by day; and we need not exaggerate it by exclaiming, in a sort of ignorant surprise, "The medicine acted like magic;" "the symptoms vanished as if by a charm." "Magic" and "charms" belong to the babyhood of science, and are instinctively rejected by the inductive mind.

Modern scepticism invades everything, and its ravages should make us determined to be exact and true. All winds and tides may bring sceptics, but they ought not to be found within our own citadel. We have, however, lately met with two such perverse instances of unbelief in eminent members of the profession that we are compelled to "show them up" for a terror to the world. Dr. T. King Chambers[1] thinks that iodide of potassium cannot cure a syphilitic sore-throat; and Dr. Bristowe[2] puts the question whether any one has seen erysipelas subside under the obvious influence of some special drug. After this, we might doubtingly ask ourselves, Did the sun rise this morning? Did we ever see any medicine do any good whatever? Distinctly and unhesitatingly we assert that syphilitic ulcers of the soft palate and tonsils can be cured by iodide of potassium, and by nothing else (save, of course, its chemical congeners, iodide of sodium or of ammonium); and within the circle of therapeutic facts we are hardly aware of any sequence more remarkable than the cure of erysipelas by tincture of perchloride of iron.[3] Now, when we recollect what was thought of erysipelas in Cullen's age; when we read such an elaborate article as that on erysipelas in Costello's inchoate 'Cyclopædia of Surgery'—a splendid monograph for its time (1841)—we ought to be abundantly thankful for the firm check which we have at the present day over one of the most ugly of spreading diseases. Why, then, all this slowness to believe? We desire to hide nothing; we

[1] 'Harveian Oration,' 1871.

[2] 'Croonian Lectures for 1872.'

[3] Dr. Russell Reynolds' "Observations on the Use of Iron in Rheumatism" may lead us to perceive some remote analogies. "Rheumatism" is a genus which will one day be broken up into species, and we shall write and speak of erysipelatous rheumatism, neurotic rheumatism, and malarious rheumatism; perhaps others also.

wish to let our foes know our weakest and worst; but, surely, we need not be ashamed of our possessions, gifts held by us in trust for the benefit of all humanity.

We close our retrospect with sincere congratulations to our fellow-labourers, and with pleasing hopes for the future. Let another decade produce the same fruit as the last and therapeutics will approach the rank of positive science. Our business is to work on, and to work ever; to help in plucking out the roots and seeds of danger and death which infest the human race, and to plant every man, woman, and child in the conditions most favorable to health and longevity. Pseudo-philosophers may sneer, anti-vaccinators may hinder the "good day coming;" but the confraternity of healers can never be daunted or diminished. The physician's calling, lauded in Holy Writ, sung by poets, celebrated by historians, will ever receive its meed of glory in all times and among all nations:

> "A wise physician, skill'd our wounds to heal,
> Is more than armies to the common weal."

VIII.—Operation Statistics.[1]

THERE can be no doubt about the value of statistics. It does not require the action of a statistical society, with its large and cogent conclusions, to convince us of this. The statistical method of inquiry is now accepted as one of great use in all departments of research. An appeal is made to the inexorable logic of facts, and an array of statistics is brought forward which seems to settle the question. And yet there is in the public mind a very general distrust of the conclusions which are thus arrived at. It is said that figures may be manipulated so as to make them prove anything that the advocate wishes, and that it is as easy to arrange figures in tables as it is to place the pieces on a chessboard. Certainly there is ground for distrust when we observe what very different conclusions are drawn from the same facts, statistically considered. Indeed, the statistical method is one which is open to many fallacies; yet where these fallacies are carefully avoided, it is

[1] 1. *Observations et Statistiques pour Servir à l'histoire des Amputations.* Par le Dr. L. ROBUCHON. Paris, 1872. P. 74.

2. *Clinical Surgical Report for the year* 1871. By GEORGE BUCHANAN, A.M., M.D., Surgeon and Lecturer on Clinical Surgery, Glasgow Royal Infirmary. P. 11.

3. *Hospital Report for the year* 1871. By JAMES MORTON, M.D., Surgeon and Clinical Lecturer, Glasgow Royal Infirmary. P. 16.

as cogent as a demonstration; but to avoid them is no easy matter. When the question that is to be judged by statistics is at all a complex one, the pitfalls become proportionately more numerous, and the danger of falling into them more imminent. Now the questions which surgery proposes to the statistician are, most of them, remarkably complex. Slight variations in the direction of a fracture, or in the extent of a contusion, or, above all, in that subtle thing which we call the constitution of a patient, may make all the difference in the result of the case. Upon such apparently trifling matters may hang the issues of success or non-success, of life or death. Yet these minute variations are beyond our power to estimate. What, for example, do we know about the differences in the natural constitutions of our patients? In old books belonging to the beginning of this century and before that date, it is common to read that the patient was of a sanguine, a bilious, a phlegmatic temperament, and so forth. But these terms convey no very accurate meaning, and we derive no practical advantage from setting down one individual as sanguine, and another as phlegmatic. It is wiser not to catalogue our patients at all, than to attempt to divide them according to peculiarities about which we know little or nothing. To classify the different varieties of natural constitution, to try to determine their respective liability to various forms of disease, and the general rules of treatment applicable to each, would be a work worthy of a great mind, and of the labours of a lifetime. If even an approach could be made to something like certainty upon this subject, it would go far to give precision to our practice and value to our statistical conclusions. It is because things are compared which are not essentially comparable one with another that so much doubt rests upon statistics. And until our knowledge is mnch more advanced than it is, not merely with regard to the varieties of human constitutions, but also upon many other points, it will be impossible for us to arrange cases in such groups as safely to draw minute statistical conclusions from them. Upon some large and elementary questions health statistics are, no doubt, even now of great value, and have done much to impress certain broad general truths upon the public mind. But when we go further than this, when we ask for precise information with regard to the risks of various injuries, or the benefits of certain modes of treatment, we see at once how little guidance we can expect from statistical inquiries in the present state of our knowledge.

These conclusions thrust themselves upon us in reading the pamphlets whose titles stand at the head of this article. They very interesting; some of the cases referred to are rare

in the practice of surgery; but, as statistical records, they are of little or no value, and, at the best, can only be considered as a very small contribution to the statistical tables of the future. Perhaps the time may come when it will be possible to have minute and exhaustive statistical inquiries upon many points in surgery; but, at present, such pamphlets as these cannot be regarded as more than *mémoires pour servir*. Take a notable example. The so-called antiseptic method of treating wounds has now been before the profession for about ten years. It was at the Glasgow Infirmary that it was first practised, and it was from the Glasgow Infirmary that the first reports of its success were sent forth. Now, we might fairly expect that, if statistics could settle the question, the relative value of the antiseptic and the eclectic methods of treatment would long since have been decided. Yet what is the fact? That there is still the greatest diversity of opinion upon this subject, and that even at the Glasgow Infirmary it does not seem to be settled; for, while the antiseptic method is sometimes used, as we learn from these reports, it is evident that the opinion of the surgical officers is not favorable to it. They occasionally adopt it, because there are certain cases in which it appears as if it might be of advantage. But they are not slow to tell the profession that its advantages are rather apparent than real, and that in their hands it has failed even in cases to which it seemed peculiarly applicable. Thus, then, we see that in a city which is specially interested in solving the question, and which has had better opportunities for so doing than any other, the value of the antiseptic method is still a moot point. Surely, if statistics were capable of deciding complex surgical questions this one would long since have been laid at rest, and the experience of the Glasgow Infirmary would have gone far to bring about the conclusion.

Though these pamphlets are all drawn up more or less in a statistical form it is impossible to deal with them merely as a collection of statistics. Of the figures which have been collected from various countries by M. Robuchon, those which he has derived from English sources are at least ten years old, and it is scarcely necessary to point out that it is hardly fair to contrast these figures with those which have been drawn from other sources more recently. The art of constructing and ventilating hospitals, and the art of nursing, to say nothing of the healing art itself, have made, and are making, such rapid progress that statistics which are ten years old are almost out of date, and certainly do not give a correct idea of the chances of patients at the present day. Still, notwithstanding that the English statistics brought forward by M. Robuchon are not so recent as

they might have been, we have no reason to blush for them. They contrast favorably with those which he has drawn from other sources. With regard to the twenty-three cases of amputation which he details at length, and which were treated in the hospital of a French country town, it does not appear that they present any peculiar features. They comprise the same proportion of deaths and recoveries as we should expect to find among an equal number of amputations treated in any institution of the same kind elsewhere.

If we turn to the reports which have been prepared by the surgeons of the Glasgow Infirmary, we observe that there are very few classes of cases in which the numbers are sufficiently large to admit of any statistical conclusion being drawn from them. The total number of cases reported being only a little over 800, and these including the whole variety of surgical affections, it is clear that the number of examples of any one disease, or of any one operation, cannot be great. Nevertheless, there are some which, as far as they go, are interesting; and there are others which deserve a passing notice on account of their rarity in surgical practice. Thus, Dr. George Buchanan tells us that he had during the past year nine amputations in the thigh with one death. During the previous year he had the same number without any death; making the very satisfactory total of eighteen cases with only one fatal result.

Dr. James Morton gives more detail of his cases, and among them are some of considerable interest. In one instance death took place from an incised wound of the femoral vein. The case was that of a butcher, who, in attempting to stick a sheep—the animal having swerved—ran the knife into the part named, and was carried to the infirmary, a distance of more than half a mile, the blood all the time pouring from him. Styptics were applied, and he did not lose much blood while in the hospital, but no attempt was made to ligature the vessel. His habit of drinking whiskey like water did not improve his chance of recovery, though his death was due to hæmorrhage.

In a case of popliteal aneurism, which was treated by ligature in Scarpa's triangle, two small veins were divided during the operation and secured by hempen ligatures. The ends were then cut off short and the wound closed over them. The incision healed by the first intention, the ligatures were never discharged. They were not "carbolized."

In speaking of operations for malignant disease Dr. Morton says—

"That these cases, with a few exceptions, are better left alone, must be the conviction of every surgeon of experience; that is, operation does not prolong life, rather shortens it. The late

Dr. John McFarlane, of Glasgow, by tracing cases after operation, clearly showed the very trifling benefit, if any, thence arising."

This is an important point, and, if Dr. Morton's view of the matter be correct, it is very discouraging for surgery. But his opinion is in opposition to that which has been expressed by Sir James Paget and Mr. Sibley, as the result of their statistical inquiries, and we cannot but think that more reliance is to be placed upon their careful analyses of large numbers of cases, than upon Dr. Morton's general statements. There is, we believe, good ground for recommending an operation in all suitable cases; and by such a course surgeons are likely to add very considerably to the length of the patient's life, to say nothing of the freedom from pain, and the relief to the mental depression of the sufferer which such a proceeding often secures. What we want is, not to lay aside the knife altogether, but to detect the cancers in their earliest stage, to perfect the means for their complete removal, and to discriminate those cases which are most likely to be benefited by an operation from those which it is wisest to leave alone. This is the course which pathological inquiry is now taking, and it is the most hopeful course in dealing with this formidable class of diseases.

One of Dr. Morton's patients, who died of pyæmia, must have been a brave fellow, and seemed to deserve a better fate. His arm was caught in some machinery, and in order to prevent himself from being drawn in bodily, he got out his knife, and heroically cut off his arm. But, alas! this only prolonged his life for a few weeks, and he ultimately died of "toxæmia."

Such reports as these by the surgeons to the Glasgow Infirmary have, no doubt, their use, and we should be sorry to say anything which would discourage the authors. But we should certainly like to see them superseded, and rendered unnecessary by the publication of a general report embracing the statistics of the whole institution. Why should the staff, in addition to all the time and energy which they give to the treatment of the patients, be required or expected to draw up statistical tables? There ought to be a special officer—a registrar — for this purpose, and upon him should devolve the classification of the cases. This is what is now done at almost all the metropolitan hospitals, and with the best results. Though the careful registration of disease is a thing of but recent origin, it bids fair to become a most important and useful part of hospital work. As a specimen of what a report ought to be the Middlesex Hospital has furnished the best example; and we may reasonably hope that, when all the great hospitals throughout the kingdom furnish us

with equally good tables of the cases that have passed through their wards during the year, some very valuable additions will be made to our knowledge. We shall then have taken the first and most important step towards obtaining trustworthy operation statistics—statistics which will serve to guide us, not merely with regard to two or three leading principles, but also with regard to many of the details of our art.

IX.—Mitchell on Injuries of Nerves.[1]

During the year 1864 there was published in America a small book, 164 pages, on 'Gunshot Wounds and other Injuries of Nerves.' This volume is the production of a joint authorship, the writers being Drs. S. Weir Mitchell, G. R. Morehouse, and W. W. Keen, at that time acting surgeons in the United States Army.

Throughout the late lamentable civil war these gentlemen had an unparalleled opportunity of witnessing the effects of traumatic nerve injury, and the Governmental establishment of an army hospital containing 400 beds for the reception of nervous diseases, under their personal superintendance, afforded them every facility not only for recording each individual phase resulting from nerve lesions, but also the effects produced by all therapeutical and other means resorted to during the treatment of such cases.

The book we are referring to (now out of print) attracted a good deal of attention in this country both from the sad circumstances which led to the opportunity for its production, and its inherent merit as an example of accurate observation and thoughtful suggestiveness.

Last year, the first of the three authors named (Dr. Mitchell) still further added to the subject matter of this work by a paper which he contributed to the 'Reports of the United States' Sanitary Commission,' on the "Diseases of Nerves resulting from Injury," and now he favors us with what is, undoubtedly, a most complete monograph on "Injuries of Nerves, and their Consequences."

The unfortunately obscure, and apparently inexplicable, nature of many nervous disorders, the evident relation which nerve action bears to the performance of every organic function, and the influence that nerve force either has, or may be supposed to have on the nutrition of every tissue, invests any

[1] *Injuries of Nerves, and their Consequences.* By S. Weir Mitchell, M.D. Philadelphia, 1872.

contribution from competent hands, tending to advance our knowledge of neuro-pathology, with more than ordinary interest; whilst our undeniable deficiency in literary productions bearing *specially* and *fully* on nerve injuries, occasions us to hail with much satisfaction the first complete treatise on the subject the English language has been in possession of.

In the performance of the duty Dr. Mitchell has imposed upon himself, it will be found that he has been keenly alive to the responsibility attached to the large experience he has fortunately obtained, whilst his well-earned reputation as an experimental physiologist of the highest order, has been admirably maintained by the manner in which he has executed his task.

After a brief introductory chapter, which serves as the *raison d'être*, he commences his book in the conventional manner, *i. e.* with a chapter on the "Anatomy of the Nerves;" such a commencement, though stereotyped in character, is, in many instances, quite essential for arriving at a clear knowledge of the views and meaning of the author, and, when treating or discussing such a subject as "nerve injury," a subject comprising within itself the fundamental principles of histological structure, coupled with both physiological and pathological action (on all which subjects acknowledged authorities are, as yet, very far from being of one mind), it is really an absolute necessity for the writer to point out in which and what particulars he agrees, or differs, from those who have described anatomically the nervous system, and the functions that system is believed to serve.

In the present work, the chapter on the "Anatomy of the Nerves" contains scarcely anything more than a passing allusion to the general and coarser distributive anatomy, but all the more important and interesting points connected with the minute and ultimate structure of nerve matter, as well as the believed in functional relation which the primitive nerve fibril bears in regard to organs and parts—more especially with reference to motion and sensation—are described, dwelt upon, and summarized in a concise, but, at the same time, very lucid manner.

In passing, we may observe that our author speaks of having verified the opinion expressed by Fromman [1] and Grandry [2] as to the structural character presented by the axis-cylinder when treated with nitrate of silver. These authorities state that, under this treatment, transverse striæ become visible, giving to

[1] 'Virchow's Archiv.,' Band xxxi, taf. 6, figs. 11 to 16.

[2] "Récherches sur la Structure intime du Cylindre de l'Axe et des Cellules nérveuses," 'Bulletin de l'Académie Royal du Belgique,' Mars, 1868.

each primitive fibril the appearance of being composed of superimposed discs, these being separated from each other by a substance differing from the discs themselves in point of composition. If there be both longitudinal striæ, as represented by Remak,[1] Schultze, and others, and also the transverse markings as described by Fromman and Grandry, then, indeed, we have a resemblance to voluntary muscle, which is, to say the least, very remarkable.

Whilst it is certainly singular this transverse striated condition of the naked fibril should be made apparent by the process referred to, it will be well to bear in mind that this condition is *produced*, so to speak, by a particular proceeding; and that thus far, at any rate, no entirely satisfactory explanation has been given why this striation should be visible *only* under the prescribed treatment. It is, moreover, necessary we think, to direct attention to the fact that branched processes of ganglion cells, and even the cells themselves are striated in a like manner when subectjed to similar manipulation: and yet further, when naked nerve fibril is so prepared and subjected to the action of light for any moderate length of time, the striation *altogether disappears* and the whole becomes *uniformly* tinted of a brownish-black colour.

Again, when theorising on this supposed resemblance to striated muscular fibre, it should be borne in mind there are authorities (*videlicet* Montgomery[2]) who maintain that this striation of muscle is solely dependent on the necessary condition of contractility, and that when thoroughly at rest, or forcibly extended, the striæ are *not* present.

That nerves, like muscles, are capable of withstanding considerable extending force is well known, and Dr. P. Tillaux[3] found that when all else had been severed, the sciatic nerves of two fresh bodies sustained a weight of 108 to 116 pounds; but here we are not informed as to whether or not the axis cylinder fibrillæ lose their capability, under such extension, of displaying with the aid of the nitrate of silver treatment, the striæ which constitutes the resemblance to voluntary muscular fibre.

But besides, and beyond these several doubtful points, there is yet one other matter to be taken into consideration; we have recently had some very startling revelations made to us by Dr. Royston-Pigott[4] as to the error of definition common to high power objectives made by our very best makers. From Dr.

[1] 'Monatsbericht der Akademie der Wissenschaften zu Berlin,' 1853.
[2] 'Centralblatt.' Page 161. 1870.
[3] Tillaux, 'Aff. Chir. des Nerfs.' P. 11.
[4] 'Quarterly Journal of Microscopical Science,' July, 1872. Page 260.

Pigott's account, and as the result of his reasoning, it would appear we often see with such objectives that which is really not present in the object itself, and therefore, until we have object glasses so constructed as to admit of their accuracy being tested by the perfect "*agreement of the enlarged miniature with the object miniatured*," we must be careful about placing much reliance upon the correctness of the pictures presented to us by our highest microscopical powers.

Dr. Mitchell considers there is a tolerably general admission as to the manner of the centripetal origin of the nerve-tubule, namely, in the cells of the ganglia of the brain and spinal cord, and that, prior to absolutely emerging from these cells, it has neither sheath nor other covering, but consists of the single element of the axis cylinder; from this latter circumstance the inference has been drawn that this primitive fibril, or fibrillæ, for it is by no means a settled point as to whether the axis cylinder consists of one or more rods or fibres,[1] is the essential element of the "axis tubule," as it was first called by Remak,[2] and that all external coverings simply serve the purpose of protection and insulation.

The similar absence of sheath or medulla in the peripheral termination of nerves Dr. Mitchell looks upon as being also pretty well determined; but he seems to hesitate in expressing his own preferential views between the conflicting opinions existing as to the manner in which the ultimate fibre terminates—free ends or loops—within or without the sarcolemma, as regards motor nerves. He freely quotes from the writings of Langerhaus, of Beale, of Hagar, of Cohnheim, of Rougel, of Krunze, of Kühne, and others; but he does not appear to have noticed the very admirable articles, on a portion of this subject, by Dr. E. Klein, which appeared in the 'Quarterly Journal of Microscopical Science' during last year and the present.

Although not strictly appertaining to the anatomy of the nerves, that is to say, to their descriptive anatomy, Dr. Mitchell has introduced into this chapter some few observations, which admit of a practical application in connection with nerve-injury; for instance, when alluding to the purposes served by the coverings of nerves, he says:

"Besides this insurance against certain forms of disease, the structure of the nerve-fibre, and its vascular relationships, also contribute to its safety, for as each nerve-fibre has its sheath, the essential axis cylinder is well protected; while also the absence, in man at least, of blood-vessels within the delicate covering described by

[1] 'Stricker's 'Histology,' (Sydenham Society), vol. i, p. 159.
[2] 'Observationes Anatomicæ,' &c. 1838, page 2, note 2.

Robin, serves to prevent, to some extent, the access of inflammation. Thus it is that the nerves are able to traverse, uninjured, parts in divers states of degeneration or inflammation. It is also this sheathing by a non-vascular envelope that limits to a degree the changes caused by neuritis, and enables the non-vascular fibres to recover rapidly and thoroughly from such disease, because during inflammation the nerve-fibres suffer principally from pressure, owing to the effusion in and about the vascular portions of the nerve.

"The toughness and general elasticity of nerve-trunks sometimes serve a useful purpose in cases of ball-wound, and I have repeatedly seen nerves escape total destruction from missiles, simply because they were thrust aside, instead of being divided. Some of these escapes, which can be explained in no other way, are very interesting. They are most apt to occur where a missile has passed through a limb midway between two joints. On the other hand, injuries of nerves in connection with bone or near to joints, are likely to be severe and lasting, because at these joints and in these positions the nerve-trunk is more firmly anchored than elsewhere, and shares in every injury which directly affects parts in its near neighbourhood."

These two paragraphs are preceeded by an account of some experiments made on the sciatic nerve of a rabbit for the purpose of illustrating the "toughness and elasticity" of nerve-fibre; and the motive for our quoting them here is, partly to give an example of Dr. Mitchell's style of writing, and partly to justify the opinion we venture to express that they are somewhat out of place in a chapter devoted, as indicated by the heading of it, to descriptive anatomy: this objection is made the more obvious by the fact that there are sections of the work specially reserved for neuro-physiology and neuro-pathology, and such matters as the above would, in our estimation, have been better suited for either of these parts of the work than for the position which they at present, occupy.

A work of so much importance as the one we have now under our notice, will, no doubt, be frequently referred to by those seeking for knowledge on the subject of nerve injuries, and a facility for finding in each division of the book, the exact kind and character of information which the various headings would lead the reader to expect is, to our mind, a thing to be desired, and, as there can be no question that the work will run through, at least, a second edition, we trust to see this slight blemish rectified upon a future occasion.

In the last part of the present chapter Dr. Mitchell refers to the physiological chiasm of nerves in man and quotes Vulpian,[1] in corroboration of his own opinion, that the only true physiological

[1] Vulpian, 'Leçons sur la Phys. Comp.,' p. 160.

chiasm is that of the optic nerves, but, he says, "I have once or twice felt doubtful as to the strict truth of this law as applied to certain traumatic palsies of the face where, in rare cases, the motor fibres of the upper lip have appeared to me to cross over, so that irritation of the nerve on the sound side seemed to cause slight muscular contractions on the diseased side."

Dr. Mitchell claims for himself and for his friend Dr. Morehouse, the credit of having been the first to point out a physiological chiasm, other than that of the optic nerves, in a certain class of the lower animals called Chelonians; all of these he describes as having a chiasm between the two superior laryngeal nerves.

After referring to the circumstance that, in the distribution of nerve-fibres from more or less distant parts of the system, those fibres, in some localities, run together and then separate only to reunite again, thus forming by repeated communications between neighbouring branches, an intricate network, or plexus, from which finally emerge the nerve-trunks that are to be distributed to particular members, he proceeds to remark upon the fact that these interchanges bestow, apparently, no physiological property beyond that which the nerves originally possessed before such communications were established. "A knowledge of these networks (our author well observes) is essential to the study of nerve wounds, because injuries of the plexuses, especially that of the neck, are very common in war, and their arrangements enable us to explain in many cases the transmission of disease from one nerve to another, which is closely related to it in the plexus, or as its parent stem."

Some brief observations as to anomalies in the distribution of nerves, and to Mr. Hilton's admirable book on 'Pain and Rest,' conclude this chapter.

"*Neuro-physiology*" forms the subject of the next chapter (the third). The first part of it is occupied with an almost too short account of the "Varieties of Nerves;" those especially dwelt upon are such as convey outwardly moter impressions producing, as a result, either voluntary or involuntary motion; next, the sympathetic with its vaso-motor function, and then the compound system of "spinal and sympathetic," and its influence in affecting nutrition by regulating the flow of blood; but Dr. Mitchell seems unwilling to admit that an increased or diminished flow of blood is, *in itself*, sufficient to account for the various nutritive changes witnessed in cases of disease or injury. He is evidently inclined, we think, to the belief in proper trophic nerves, although admitting that the physiological groundwork is still too defective to admit of the idea being sufficiently and fairly discussed.

We have said that Dr. Mitchell is inclined to the belief in

trophic nerves, nevertheless, he points out with great fairness the circumstance that the original experiments of Samuel[1] (on which he framed his theory as to the existence of distinct nutritive nerve-fibres, that is to say, distinct from those which preside over the motion of muscles and vessels, as well as those which affect sensation), when performed by other practised experimentalists have not yielded similar results. Weber,[2] for instance, repeated all Samuel's experiments without producing, or, at any rate observing, any of the effects described by him, the faultiness, real or supposed, of Samuel's experiments having been previously pointed out by Dr. Tobias, a pupil of Virchow's.

The interest displayed of late years about this question of trophic nerves caused us to approach this portion of the work with not a little curiosity, a curiosity almost amounting to anxiety, as to the opinion that would be offered (by one who has had the large experience in nerve wounds that Dr. Mitchell has had) upon a point that has been so warmly disputed. We cannot express ourselves disappointed that Dr. Mitchell feels constrained to deliver the indefinite judgment he has done, because, whatever hopes or expectations we may have had upon the subject, we felt that a confident reliance might be placed upon the entire honesty of purpose, the carefulness and accuracy of observation, that would be brought to bear upon one of the most difficult problems the physiologist has been called upon to solve, and the circumstance of our author being apparently swayed, during his observations, now in favour of proper trophic nerves, and now in the opposite direction, only convinces us that, in spite of his earnest desire to unravel the thread, the entanglement of the whole subject is, at present, too great, the knots to be untied too many, to admit of any definite judgment being arrived at *until*, at least, the anatomical conditions of the necessary fibres have been made more apparent than the most painstaking and laborious dissections have hitherto been able to accomplish. This seems also to be Dr. Mitchell's opinion, for he remarks—

"*As yet* there is no absolute physiological proof of the existence of such nerves. *The anatomical demonstration of these fibres is equally wanting* [the italics are our own], so that at present the justification of their existence lies in an apparent necessity for their presence which so impressed Duchenne as to cause him to remark that 'if we had no knowledge of such nerves, we should be forced to invent them.'"

[1] Leipzig, 'Wigand,' 1860. 'Canst. Jahresber.,' vol. ii, pp. 53-57.
[2] 'Med. Times and Gaz.,' Oct. 1, 1864.

He then proceeds—

"If, in fact, we exclude vaso-motor influence as capable alone of explaining the pathological changes which follows nerve wounds, we are forced to fall back upon the nerves of motion and sensation, or to believe in a system of independent trophic nerves."

But then, a little further on we read—

"The phenomena of nerve wounds, as I have seen them, lend no conclusive support to the theory, and there are in them, as in many other pathological facts, certain arguments in favour of the possibility of disorders of nutrition being capable of production by the irritation of ordinary nerves of sensation, and, indeed, of motion."

We are likewise reminded that the observations of Vulpian and Phillipeaux seem to point to the probability that the nutritive conditions of the various tissues depends upon the nerves concerned in their special and particular functions: destroying a sensitive nerve, affects the skin, hair, and nails, and section of a motor nerve produces atrophy of muscles; and yet, in the face of this, we remember that Golz[1] was led by his experimental researches to arrive at the following conclusions:—That connection of the ordinary nerves of a part with the nervous centres is not absolutely necessary for the maintainance of the vegetative functions of the part. Circulation, change of arterial into venous blood, inflammation—each and all these processes take place in a limb when every known connection with the nervous centres has been completely cut off.

Dr. Mitchell also says, when dwelling on the effects produced by nerve wounds on temperature, that, at some distant period after *complete* division of nerves in a limb, there is lowering of temperature and a state of passive congestion, whilst, after a longer period of time, there is evidence of greatly lessened blood supply; nevertheless, under even such circumstances, although the life of the limb may appear to be lowered, and any external part of it may be easily injured, *yet repair readily takes place*, sometimes perhaps slowly, but in other cases the restoration may be unusually rapid.

He adds—

"I have watched many such cases of complete separation of a limb, or of parts of a limb, from all neural influence and have failed to see any inevitable consequence except general atrophy, with usually muscular contractions, and a sallow, unhealthy look of skin, which is apt to become scaly and rough. *The nails grow as usual, perhaps a little more slowly; and the hair is commonly unchanged*" (our italics).

These extracts from the work are given essentially for the

[1] Virchow's 'Archiv.,' vol. xxiii, p. 451.

purpose of showing the conflict of reasoning which exists in Dr. Mitchell's mind (touching the nerves of nutrition), resulting, almost as a necessary consequence, from his observations on the effect produced by nerve injuries on the nutrition of tissues; still, when concluding this part of his subject he is able to present us with certain very interesting and useful generalisations:

"Total section of the main nerves of a limb results invariably in atrophy, but not necessarily in inflammatory conditions. It does not, however, forbid these, nor is repair always interfered with to any great extent.

"Partial wounds of nerves, and especially gun-shot lesions, are apt to give rise to a large number of trophic changes in the skin, hair, nails, areolar tissue and muscles.

"Except the entire arrest, for a time, of nail-growth, every trophic alteration capable of arising from injuries or diseases of the centres is also to be met with as a consequence of the wounds of nerves. Sections of muscle-nerves causes atrophy and contraction of the related muscles. Partial wounds occasion various degrees of wasting, with more or less loss of muscular sensation, and impairment or loss of power to respond to electric, galvanic, or mechanical irritation.

"Exceptional cases exist of atrophy without defect of excitability under stimulation. Trophic changes in the skin, hair, and nails are never present after nerve wounds without some affection of sensation, such as dysæsthesia, anæsthesia, or hyperæsthesia; but any of these latter states may exist without the nutritive alteration in question. Trophic changes are most prone to follow wounds of nerves which are distributed to the hands and feet, and more rarely occur when the injury has involved only the nerve branches which supply the upper portions of a limb."

Apropos of this matter of nutrition, although distinct and altogether separate from the volume we have under consideration, it may be permitted us to observe that Dr. Mitchell has very recently arrived at what promises to be a most important piece of differential diagnosis, viz. that "in all sudden cerebral palsies the nails cease to grow. In hysterical palsies of one limb, or both, whether paraplegic or hemiplegic, the rate of nail-growth is unaltered—

"I published last year, in the 'Summary of the Proceedings of the Philadelphia College of Physicians,' a brief communication, in which I pointed out the remarkable fact that any sudden cerebral palsy arrested the growth of the nails on the palsied side. Since then I have studied the subject with care, and have learned certain facts, which I desire to make public in advance of a paper which will treat more fully of the nutrition of paralysed limbs.

"The facts which are now known to me may be very shortly

stated. The growth of nails is usually retarded more or less in all palsied limbs, whether the palsying cause be spinal, cerebral, or belonging to a nerve-trunk. As yet, owing to want of opportunity, I do not know whether or not there is any complete temporary arrest of growth in spinal or peripheral disease and injury. As regards the latter, I am disposed to believe there is not an arrest, but only more or less retarding of growth and deformation.

In cerebral palsies, whether from clot or embolus, there is an entire cessation of nail-growth on the palsied side. Usually when they begin to grow again it is a sign that the power of movement will also improve within a few days. The rate of growth slowly increases, but it usually requires four or five months for such nails to produce an entire length from matrix to free edge. To study the change, I stain the nails of both sides with nitrate of silver or nitric acid; the latter is preferable, because it soaks into and stains of a deepening yellow the whole thickness of the nail. Staining is not, however, essential except for comparison, because the line of arrested growth is marked by a deep groove, which for months may be seen as it passes down the nail, so that when accustomed to the rate of growth the place of this furrow will enable an observer to guess pretty well at the date of the attack of paralysis. The palsy need not be complete to cause this arrest. It is found in cases involving either cerebral or motor palsy or sensori-motor paralysis, but as yet I do not know whether or not in the rare cases of pure sensorial palsies of cerebral origin it also exists, nor as yet have I any experience which enables me to say whether or not, in sudden spinal palsies, there is also complete cessation of nail-growth.

These observations have naturally led me to a close study of the nutritive changes as regards growth and repair of hair and skin in the cases alluded to, but as yet I am hardly ready to speak with confidence upon subjects such as these, which promise to open a rich field in differential diagnosis. It seems to me possible that the nail-growth may not be altered in the same degree by lesions of the cerebrum, cerebellum, pons, and corpus striatum; and I have some observations which appear to point hopefully to these facts of nail-growth, as a future means of aiding us to tell what parts of the brain have been attacked.

Very recently, one distinct, and, as I believe, most valuable, practical contribution to diagnosis has come out of my observations. It is briefly that—

"*In all sudden cerebral palsies the nails cease to grow. In hysterical palsies of one limb, or both, whether paraplegic or hemiplegic, the rate of nail-growth is unaltered.*

"This point was first determined in a case which I saw in consultation with Drs. Koerper and Frické. In a young girl, a long series of hysterical phenomena ended in complete sensori-motor hemiplegia. The nails, being stained, were found to grow equally on both sides. In a later case, seen by my friend Dr. Packard, the

hysterical palsy was confined to the left arm, and the nails grew alike on the two sides.

"A third case was that of a young married lady who had had many hysterical attacks. She was scared by a wild cow in the street, and, after a cataleptic fit, lost the use of her left arm. Sensation was slightly altered. The nails grew at the same rate on both sides.

"Two days later I saw a middle-aged spinster, who four years before had had a slight attack of right hemiplegia, from which she perfectly recovered. She has mitral disease, and is a most likely person to suffer from palsy. On the 10th of April, 1872, she received a letter which greatly distressed her, and, in consequence, was seized with a twitching of the left lower eyelid. A few hours later she became by degrees paralysed as to motion and sensation on the whole left side. Many of the usual manifestations of hysterical palsy were wanting, and the previous history and the cardiac conditions were such as to make probable an organic cause. I stained the nails on the second day, and although the palsy was unaltered a week later, the nails on both sides were growing. A few days after it was clear that the rate of growth was the same, and I therefore ventured to assert that the case was in origin purely hysterical. A very speedy and complete recovery under appropriate treatment verified my prognosis.

"I trust that I have said enough to make it appear that if I am correct in my observations, they promise to afford no unimportant addition to our means of discriminating between palsies of functional and of organic origin."

"*Nerves of Pain*" are dismissed in a very few short paragraphs, Dr. Mitchell appearing unwilling to assent to the idea of there being any such special nerves, that is to say, he does not recognise *pain as a distinct sense*, with afferent nerve tracts peculiar to itself; he contends that, as impressions made on sensory nerves of special sense may rise to the pitch of being painful, it is in the highest degree probable that pain is simply the expression of a certain amount of irritation in any centripetal nerve.

"*Recurrent sensibility*," another division of this chapter, is passed over in a still more brief manner, but the important views and speculations of Bernard, Gubler, Brown-Séquard, and Vulpian are referred to, whilst the fact that no evidence of recurrent sensibility has been discovered in fishes and pigeons is mentioned as an argument against the necessity for such a physiological arrangement. Dr. Mitchell also failed to detect anything like "recurrent sensibility" in ducks and chickens, and also in large chelonians (*Chelonura serpentina*), although every precaution was taken to ensure the success of his experiments.

"*Neurility and Neural Excitants*."—The consideration of

these occupies no inconsiderable portion of the chapter, and we regret our inability to follow the writer to the extent we should wish to do, and to the extent which the interest and importance of the subject merits; but there is still so much to be noticed in this attractive book that, however reluctantly, we are compelled from sheer want of space to pass over in a hurried manner many things we could wish to dwell upon.

The identity of "nerve force" with the "electric current" meets a strong opponent in this volume. Dr. Mitchell considers that, if for no other reason, the difference in the rate of transmission is, in itself, sufficient to distinguish the one from the other: based only upon this, we should certainly feel ourselves scarcely justified in arriving at such a conclusion, the more especially as the experiments of M. Gaugain, alluded to in this chapter, prove to demonstration that the electric current does undergo a process of "slowing" in passing through moistened thread; hence a very natural question arises, whether the resistance to the electric current which we know to be greater in some bodies than in others, may not, in the case of the nerves, be simply an exemplification of the same well-known fact. What may be the necessary modifications of molecular arrangement by which the effect is produced we, at this time, have no absolute knowledge, and the determining condition will, perhaps, set at defiance all known means of experimental research, but we cannot recognise that in the circumstance of nerve force moving in man at the rate of 308·8 feet[1] a second, whilst electricity has been estimated to travel at a rate of 462,000,000 feet per second, we have *all* that is required to definitely disassociate and distinguish these forces.

This rate of nerve force has received a good deal of attention from our author, and he believes a correct knowledge of this point has a real practical value in determining the possible seat of some initial injuries.

With regard to "*neurility*," this is spoken of as being common to all nerves, though not to all nerves alike, each being influenced by its own physiological properties. Dr. Mitchell looks upon the "*vis nervosa*" of sensitive or motor nerves as not varying in its nature, and draws an inference that as the anatomical similarity of nerve-fibres is very great, there can be but little difference in point of physiological effects. As illustrative of this, he says that when sensitive or afferent nerves are irritated or stimulated naturally, the message, so to call it, is carried centripetally; while with motor nerves the opposite is the case, but this propagation in one direction is merely a matter of

[1] Dr. Kohlrausch, 'Henle und Pfeiffer's Zeits. f. Rat. Med.,' Bd. 28, 1866, p. 190.

appearance, caused by the circumstance that it is only at one end that either set of nerves is provided with organs specially adapted to announce the reception of an impression. In short, there are many conditions which serve to show that the nerve current is extended in both directions, *i. e.* central *and* peripheral; and it may even be said to have been established by the researches of Vulpian, Phillipeaux, M. Bert, Du Bois Reymond, and others, that if sensitive nerves are connected with motor centres, or the reverse, the special influence of each will, upon excitation, be transmitted; in other words, ordinary sensitive nerve-fibres are capable of conveying motor influences, and motor nerves may afford passage to sensitive impressions; these facts we shall find to be fully utilised when we arrive at the practical portion of the work.

Neural Stimulants.—This forms the last division of the chapter, and treats of all those stimuli which may affect abnormally the nervous system generally: the volitional stimuli, and those resulting from the action of external agencies on the sensory nerves are excluded from consideration. The effects produced by disease, either on the central, terminal, or intermediate part of nerves are pointed out, and the views and opinions of Budge, Kölliker, Kühne, Bernard, and Waller are freely but concisely referred to. We said that the subject of "*neural stimulants*" forms the last division of the chapter; but this is scarcely accurate, as the last few paragraphs are occupied with some remarks upon the effects produced by extremes of "*heat and cold*" on nerve tissue.

Chapter IV is headed "*Physiological Pathology of Nerve Lesions;*" and here, without wishing to push criticism to an extremity, we are disposed to ask—Is there, can there be, such a thing as physiological pathology? Must it not be that πάθος and φύσις, are at variance? Is disease natural? Death may be, nay, is natural; but disease would seem to be Nature's antithesis. We know it has been said that disease is *a manner* of living, but it surely is an unnatural manner; and while physiology would point to healthy (natural) function, pathology can only be correctly applicable to disordered (unnatural) function; therefore, although knowing quite well the light in which Dr. Mitchell wishes these two words to be received, we nevertheless look upon their juxtaposition as unfortunate, and are of opinion that the phrase *the pathology of nerve-lesions* would have more surely (and at the same time sufficiently) defined the contents of the chapter.

Whilst with physical forces generally we are in the position of being able to recognise them only by their effects; that is to say, when speaking of centripetal and centrifugal force—of heat,

light, and electricity—we know not of what, or in what they consist—what, indeed, in their very essence, they really are—so with regard to nerve-force, nerve-function, both physiological and pathological, we take cognizance of the condition, we estimate the function by its effects, without being able to comprehend the absolute essential elements of its existence; and in no part of the system of organized beings is there greater need of research for the elucidation of a whole train of phenomena hitherto not only not understood, but, there is too much reason to think, frequently misunderstood. To minds finite in their operation it would assuredly be unreasonable to expect that which can only be reached through the infinite, but it is *not* unreasonable to expect that those aberrations from the natural which we characterise as pathological may, by patient and persevering research, be revealed to us; and that being so, we shall not only be able to place a correct value on the symptoms and conditions continually presented to our observation, but, with a knowledge of the producing causes, we shall be able to give a rational account of and to treat in a rational manner many ailments which now we are obliged to speak of in a vague way as *nervous disorders*, and to treat with the most unsatisfactory empiricism.

When opening the present chapter, Professor Mitchell says—

"In the different forms of mechanical injury of nerves, a certain share of functional loss depends upon the cutting, tearing, stretching, or bruising of nerve-tubes which the missile or weapon occasions. To this set of incapacitating causes are soon added, in varying degrees and at more and more remote periods, congestion, inflammation, and sclerosis, so that the remote pathological consequences are very often more serious than the primary hurt. Because, in the study of symptoms and of the varieties of nerve lesion, it becomes difficult or impossible to discern what are due to mechanical interference, and what to the sequent pathological states, it is desirable to view these latter separately, and also to study the process by which an isolated nerve alters (degeneration), and that by which in time it is repaired (regeneration). For whatever so affects the nerve-tubes as to annihilate their power to carry impressions, whether this be mechanical, as in wounds, or a pathological state, such as inflammation or sclerosis, inevitably dooms them to a gradual change, which results first, in their partial destruction, and possibly, after a time, in their more or less complete repair."

The strong practical sense contained in this paragraph will commend itself to all thoughtful practitioners. Truly such a division as here laid down would seem to be an essential pro-

ceeding for the study of neuro-pathology, and we will presume somewhat upon our licence as critics to say such a study should comprise within itself, over and beyond the points mentioned, a thorough comprehension of the broad *distinction*, and yet intimate *connection*, which exists between pathology and morbid anatomy—between an alteration of ultimate structure which ensures, either annihilation of function or the permanent imperfection of its performance, and that more temporary and indistinguishable alteration of molecular arrangement which may determine functional derangement, but which also allows of its subsequent perfect resumption.

Upon the subject of *congestion of nerves* we are told, and correctly told, that our clinical knowledge as to the congestion of nerve-trunks is so limited that very few pathologists would be able to describe and define the symptoms indicative of its presence, and then, with considerable modesty, Dr. Mitchell proceeds to relate a series of original experiments which he instituted with the view of letting light into this dark corner of our neuro-pathology. We would that we could afford space to recount these experiments and the conclusions arrived at, but we are warned to hurry on.

Acute Neuritis is next spoken of and illustrative cases given, but we are disposed to take exception to the introduction of *treatment* in this part of the work.

Chronic Neuritis, *Regeneration of Nerves*, and *Autogenetic Regeneration of Nerves*, form the remaining divisions of the chapter, and upon each of these much useful information is given.

Chapter V relates the "Varieties of Mechanical Injuries of Nerves." Although a chapter of considerable interest, and very necessarily introduced in such a work, it is scarcely one which admits of the introduction of much new matter or, indeed, of more than a recapitulation of the many and sometimes very extraordinary kind of nerve injuries that may be received, either as the result of accident, or by the use of weapons used in warfare. With regard to the effects of division of nerves by incised wounds, Dr. Mitchell's experience is in accord with that of most practitioners who have seen much of such injuries, viz. that although there is a pretty general impression that a clean-cut nerve, in which the division is either partial or complete, *usually* has its function perfectly restored, the opposite to this is but too frequently the case; indeed, Dr. Mitchell declares that, after much experience, he has found the recoveries to be, using his own words, "*sadly rare*," and he refers to the experience of Mr. J. Hutchinson, given in the 'London Hospital Reports' for 1866, as confirmatory of his own,

A rather long reference (illustrative) is made to the disastrous effects that have sometimes ensued from lancet wounds of superficial nerves, such as the musculo-cutaneous or the anterior branch of the internal cutaneous nerves, which occasionally happened when bleeding was more in vogue than it is at present. As a case in point, he quotes from the complete works of Ambrose Paré, edited by Malgaigne, and published at Paris in 1840. The case is that of Charles the Ninth, and the following is the account given by his attendant:

"Or pour instruire jeune chirurgien, et le dresser mieux à la pratique dessus-dite, je reciterai cette histoire, qui n'est hors de propos pour la curation des piqueures des nerfs.

"Le Roy ayant la fiévre, Monsieur Chapelain, son prémier médecin, et Monsieur Castelan, aussi médecin de sa majesté, et premier de la Royne sa mère, luy ordonnerent la saignée; et pour la faire on appella un qui avoit le bruit de bien saigner, lequel cuidant faire ouverture à la veine, piqua le nerf; qui fit promptement escrier le Roy, disant avoir senti une très grande douleur."

Subsequently contraction of the muscles occurred, the pain continuing, and for three months the king could neither bend nor extend the arm.

Attention is called to the fact that slight injuries to the digital nerves are particularly prone to occasion wide-spread reflex sympathies. A communication of Mr. W. M. Banks', which appeared in the 'Liverpool Medical and Surgical Reports,' October, 1869, vol. iii, p. 73, is alluded to as containing an account of cases of this kind, and in which Mr. Bickersteth removed portions of the affected nerves with the very best results.

Punctured wounds of nerves.—There must be something special, something altogether beyond our present comprehension, as regards the effects produced by these wounds, for whilst, in some instances, nerves are punctured without producing any very obvious pathological effect, as, for instance, in acupuncture, yet in other instances the most destructive changes are occasioned. Billroth remarks at page 117 of his 'Surgical Pathology" (American translation) that "punctured wounds of nerves naturally induce, according to their extent, paralysis of variable amount, otherwise they have the same effect as incised wounds of the nerves." Dr. Mitchell observes that wounds of the nerves deserving of being called "punctured" are very rare, but are apt to be followed by very serious "accidents, owing to secondary changes in the nerve, and occasionally also by grave constitutional disturbances." The views of Bérard, Roux, and Descot are brought forward to the like effect.

Contusion.—Hitherto it appears that the only experimental

research in regard to this kind of nerve injury has been made by Tillaux; the conclusion arrived at by this observer, and corroborated by Dr. Mitchell in his subsequent experiments, is that, according to the amount of injury, a so-called fatty alteration of the nerve takes place, progressing exactly as is found to be the case in section of nerves.

Symptoms and results of contusions.—We have not here any strikingly new observations to detain us: illustrative cases are given, and the opinions of well-known and highly esteemed authorities are brought forward, bearing particularly upon such points as naturally have most interest for surgical practitioners. We may mention amongst these authorities the contributions of Cansard, 'Essai sur la Paralysie suite de Contusion des Nerfs,' 1861; Descot, 'Affections locales des Nerfs,' Paris, 1825; Hamilton, 'Archiv. Gén. de Méd.,' 1838, t. ii; Desault, 'Œuvres Compl.,' t. i, p. 335; Duchenne, 'Mém. de la Soc. de Biologie,' 1854, p. 119; E. Le Bret; and there are many others, not forgetting several of our own countrymen.

Compression.—The following sentence commences this division:—"Either from external or internal causes, the nerves of the human frame are subject to a great variety of modes of pressure, yet very little attention has been given to the nature of the changes which they bring about, and still less to the difference in symptoms which arises from the varying rate with which the pressure is applied."

After detailing the experiments performed by A. Waller, Bastien, Vulpian, and others, we have a record of Dr. Mitchell's own experiments. As evidence of the careful and able manner in which our author pursues his investigations, we shall quote freely from this portion of the work, first expressing a hope that an account of these experiments may not be without interest. He proceeds:

"A tube of glass, enlarged a little at the extremities, one tenth of an inch in diameter and twenty-one inches long, was fitted at one end with a thin caoutchouc cover. Having laid bare the sciatic nerve of a rabbit, and slipped under it a thin cork, I allowed the bladder closing the tube to press lightly on the nerve-trunk, the lateral expansion of the bag being limited by a ring of cork, nicked below so as to rest on the flat cork and yet to allow of the passage of the nerve. Two inches of mercury were then poured into the tube, so tilted as not to permit of the nerve being hurt by the fall of this heavy metal. The tube was next raised to the perpendicular position. The first effect was to cause slight twitching and some expression of pain. To save pain and to leave only the better test of muscular movement, I divided the nerve an inch from the tube on the central side. The quantity of quicksilver was now slowly in-

creased. Each increment was apt to produce new twitches, as was also any sudden movement of the tube; but constant pressure, like an uninterrupted galvanic current, gave, after the first movements, no further result. As each half inch or more of mercury was added to the column, I tested the nerve from time to time on the central side with a current from a small single Smee cell. The communication seemed to be unbroken, and muscular movements could thus be excited until I had put on the nerve a pressure of twenty inches of mercury. Ten or twelve seconds of this pressure gradually lessened and at last abolished, the power of the nerve to carry to the muscles the impressions made by the galvanism.

"The tube was then lifted carefully, when to my surprise (and great interest) I found a gradual return of this power of transmission, although ten or fifteen seconds elapsed before the first visible manifestations occurred."

Some further particulars are then given as to the animals operated on, and then Dr. Mitchell proceeds to observe—

"It seems impossible to look upon this as other than a mere mechanical disturbance of the tubal contents, and a like mechanical restoration of their conditions for activity."

It is needless to speculate here upon the very suggestive character of these observations in a physiological point of view. Pathologically considered, they are not less interesting.

The above, and other ingeniously devised experiments, serve admirably as the starting-point on which to base sound conclusions as to the effect of pressure on nerve tissue under a variety if not under all conditions, and, to a very great extent, this principle is made applicable in the numerous divisions and kinds of pressure which have previously been, and are subsequently described.

We find the 6th Chapter devoted to the "Symptomatology of Nerve Lesions." The first division of this part is taken up with a study of the value of local symptoms, and here the author's experience in the matter of gunshot wounds is again largely called upon. The symptoms of nerve injury so inflicted, we are told, are, generally speaking, local and general. During an inquiry made in hundreds of cases where nerve-trunks had been injured, the most singular diversity of account was given as to the first sensations felt, some saying they felt as if simply struck by stick or stone, others complaining of immediate and intense pain both at the wounded point and along the course of the nerve-tracks, but, as a rule, the pain at first was not severe.

In the work mentioned at the commencement of this notice —'Gunshot Wounds and Injuries of the Nerves'—forty-three cases of nerve wounds are given and analysed as to the imme-

diate symptoms. To this number Dr. Mitchell has now added forty-eight cases; out of these ninety-one cases more than a third had no pain at first, and very many did not know they were shot at all until made aware of the fact by weakness, or the sight of their own blood.

It is thought that this difference as to pain may, to some extent, depend upon the velocity at which the missile is travelling, when moving with comparative slowness occasioning the most pain. The idea of Dr. Richardson is here brought in, namely, the possibility of opening abscesses, &c., by means of a rapidly revolving knife.

The effect produced on nerves of mixed function, when injured by violence, is by no means uniform; most frequently the impression seems to be principally felt by the sensory branches, whilst motor defect usually occurs as the result of secondary changes, but, when very grave injuries are produced by bullet-wounds, both sensation and motion are equally affected in the first instance, so that, directly after the receipt of the injury, it is not always easy to say whether the nerve has been completely divided or merely stunned, and thus it sometimes happens that after what seemed at first a very grave lesion, the functions of either sensation or motion, or of both sensation and motion, are restored after the lapse of a time, uncertain, of course, as to its length. But sometimes these wounds produce the very opposite to loss of motion; indeed, the most violent spasmodic contraction is occasioned, and, in a case we have related to us, a soldier who was wounded in the brachial plexus at the battle of Antietum was obliged to ask a comrade to unclasp his fingers from his musket, so rigidly and firmly had they got hold of it.

The division of this chapter on "*Early Constitutional Symptoms; Shock,*" is a very important one, and is replete with such interesting matter we feel almost vexed the necessities of "space" should so inexorably compel us to give the subject that which it certainly does not deserve, a mere hurried and passing allusion. We experience, however, much satisfaction in the belief that most of our readers will take an early opportunity of possessing themselves of the volume, and thus realise by their perusal of it the same gratification that we have derived.

We cannot pass completely from this subject of early constitutional symptoms without giving Dr. Mitchell's definition of "shock," and we are disposed to do this the more particularly because in his theory of it he takes objection to the views upon the subject which have been promulgated by Brown-Séquard and others.

"Shock then" (says Dr. Mitchell) "is reflex disturbance, or, in

some cases, paralysis of centres. Why in one case the cerebrum should suffer, in another the heart, and in a third the motor centres of the leg or arm, is as yet inscrutable. A ball crushes a nerve, and the tremendous shock instantly propagated to the spine falls ruinously upon some one of the numerous ganglia through which it travels. Is this because it finds a weak point, or is it that conduction checked somewhere causes at that spot destruction from dangerous accumulations of nerve force? Tempting analogies here open to certain electrical phenomena, but as yet we lack such exact knowledge as would justify further inferences."

Dr. Mitchell's objections to Brown-Séquard's theory are fully stated in the 'New York Medical Journal,' 1866, the article being called "Paralysis from Peripheral Irritation."

A number of cases are related as being demonstrative of the views of our author as to the true nature of shock.

Remote symptoms form the subject of the 7th Chapter. At the head of these Dr. Mitchell places tetanus and chorea. The prevalent belief is mentioned that slight wounds of large nerves are more prone to give origin to tetanus, but the opposite to this is absolutely the fact, the initiation of the condition being the result, in certainly the majority of cases, of irritation of the extreme distribution of nerves, and where there has been no preceding injury to any large nerve-trunk. In 200 instances in which large nerve-trunks were wounded, and the cases under our author's own immediate observation, not a single case of "lock-jaw" occurred.

Chorea is not considered to be by any means a frequent consequence of nerve wounds, although there are recorded several cases in which it took place.

Later local symptoms.—The most important of these appear to be most certainly those which come under the head of "trophic changes." Changes affecting the skin and appendages, changes affecting the connective tissues and the nutrition of joints, are all brought under notice, and cases bearing upon these various conditions are given to enable the reader to realise precisely the effects produced.

"The Influence of Nerve Wounds on Secretion," and the "Effect of Nerve Wounds on Calorification" bring the chapter to its conclusion.

The two next chapters, VIII and IX, are dedicated, the former to "Sensory Lesions," the latter to "Diagnosis and Prognosis of Injuries of Nerves."

The manner in which *sensory lesions* are made manifest is either by increased, diminished, or perverted sensations, the two former, varying in degree, from the most exquisite sense of pain to the utter absence of any feeling whatever, and in the case of

"perversion" the sensations being frequently so extraordinary as to set at defiance anything like description.

The state of hyperæsthesia Dr. Mitchell has never found existing as an *immediate* effect of nerve injury; it may, he says, come on a few days after a nerve has been wounded; but, he observes, "when, indeed, there is hyperæsthesia for pain we are apt to find it associated with lessened or lost power of tactile appreciation, and this rather because of the confusing influence of pain than necessarily from actual loss of tactile appreciation."

All cases of "glossy skin" with causalgia, are tolerably sure to exhibit hyperæsthesia; but there may be, and there, indeed, are, recorded cases of hyperæsthesia and burning pain without "glossy skin." In the book on nerve wounds by Drs. Mitchell, Moorhouse, and Keen, a case of hyperæsthesia of the thigh is related in which there was neither glossy skin nor burning pain, and most surgeons who have had a large experience in nerve wounds will, no doubt, be able to call to mind instances in which there has been localised hyperæsthesia in regions *remote* from the wounds inflicted.

Our attention is next directed to "Muscular Hyperæsthesia," and the importance of distinguishing between it and the same state of the skin. In such cases the skin admits of being pinched slightly without communicating any unpleasant sensation; but deep pressure, so as to reach below the surface, detects soreness, and movement of the parts beneath is almost always painful: gunshot wound, blows on the back producing injury of the outgoing nerves, and spinal concussion, are the general causes of this sensory lesion.

"*Defects of Sensation*" (anæsthesia). These can only be properly estimated by a thorough and proper examination of the skin, aided by the use of particular instruments.

According to Dr. Mitchell's experience, there is but one part of the skin in which the process of localising sensations seems particularly deficient, viz. the toes. He has noticed in labouring people—and we take it for granted the observation would hold equally good in regard to other than labouring people—that in touching their toes—their eyes, we conclude, being shaded—the third toe is mistaken for the fourth, and this for the third; more rarely the fourth is mistaken for the fifth or little toe, but this is very rarely taken for the fourth.

The instruments used in detecting the various anæsthetic states are the æsthesimometer, which is simply a pair of compasses, the points being covered with little rounded balls, and a scale placed above, or a graduated bar, upon which the two limbs slide, by which means it can be determined how far apart,

or rather how little apart, the points admit of being separated so as to be distinguishable; a needle for the detection of pain, and the electric brush.

This part of the work is very full of illustrative cases, but we cannot help looking upon them as being a little out of place here, or perhaps we ought to say they are rather distracting; besides, some of them reappear again when we arrive at the three chapters on treatment.

"*The diagnosis of injuries of nerves*" cannot present, and Dr. Mitchell tells us does not present, any considerable difficulty—we mean, in the earlier stages; but later on, and in some few rare cases, the diagnosis is somewhat masked, in consequence of the reflex disturbance of remote nerves, a subacute inflammatory condition setting in, or it may even be a general sclerotic state passing centrally from the nerve first affected, thus implicating one, or perhaps several, cords of the parent plexus.

"*Prognosis of injuries of nerves.*"—As will be readily understood, in every case of grave nerve lesion the ultimate and perfect recovery of function is an extremely doubtful matter. It is said that a severe contusion is more likely to give rise to prolonged and, perhaps, permanent injury, than any other form of mechanical disturbance. Why it should be so is not, at first thought, at all clear; but when, in a former part of the work, drawing, or endeavouring to draw, some positive distinction between electricity and nerve-force, it is stated that "bruising a nerve" effectually prevents the transmission of any nervous effect, we at once recognise the probable serious effects of severe contusion.

It is often of the utmost importance, in a case of severe injury to a large nerve, that we should be able to form some opinion, of a reliable character, as to what will be the probable fate of the parts over which such nerve exercises its influence.

After stating that no early opinion is of value, Dr. Mitchell thus expresses himself:

"To determine the future fate of the limb, we resort to the induced and galvanic currents. If, according to Duchenne, a muscle moves ever so little when paralysed at a period thus remote from the wound, there is a fair chance of recovery—a chance which lessens in proportion to the difficulty with which it contracts under this stimulus. If, however, a muscle neither moves nor feels the pain of the current, this is an indication of so complete a separation from the centre that from three months to a year may elapse before the probability of nerve destruction becomes such as may enable us to call the part into action. This refusal to respond to the faradic current does not imply a certain loss of inability to act under galvanism or of such mechanical irritation as a smart tap. In grave

cases, however, there comes a period when the muscle may be dead to every form of excitation, and even to the direct effects of galvano-puncture.

"The sensibility is to be examined with the electric brush, and if found, after two or three sittings, to be dead to this most intense irritant, we may safely conclude that the nerves of the part are absolutely cut off from their central connections and will require long periods for their repair, should this ever be possible."

We give this account of Dr. Mitchell's, not as possessing any particular novelty, or, indeed, as differing in any way from the proceeding that would be adopted by our own scientific surgeons, but simply as the utterance of one who has had an immense field for observation and who informs the profession of the course he has pursued for arriving at a satisfactory prognosis—a course which he and his colleagues have tested sufficiently to be able to rely upon it with the utmost confidence.

The three following chapters, X, XI, XII, are headed "Treatment," and are, more or less, confined to this subject. The thoroughly clinical characters of this division of the book scarcely admits either of abstract or analysis, but the reader will find in it a careful and intelligent record of the most instructive cases, coupled with an accurate and detailed account of all the means made use of, whether for cure or relief, and, what is of the highest importance, an honest narrative as to the result effected by each remedy employed.

Some of the cases are terrible from their severity, and the good effect of treatment so much greater than, from the graphic description given of the deplorable condition of the individuals, one could have at all expected, that we are compelled to believe it is justifiable to hold out "hope" in almost the *very worst* instances of nerve injury, and, for our own encouragement, to bear in minds the lines of Richard Alison:

> "In hope a king doth go to war,
> In hope a lover lives full long,
> In hope a merchant sails full far,
> In hope just men do suffer wrong,
> In hope the ploughman sows his seed;
> Thus hope helps thousands at their need.
> Then, faint not, heart, among the rest,
> Whatever chance, hope thou the best."

One of the cases related (No. 51) was of so great severity, and yet, eventually, terminated in a manner so infinitely more favorable than any one would, at the onset, have been at all warranted in predicating, that we would direct particular attention to it.

The book contains two more chapters, one on "Lesions of Special Nerves," the other on "Neural Maladies of Stumps."

Both contain matter equally interesting and important, but we feel we shall best promote the true advantage of our readers by recommending them to lose no time in consulting the work itself, as by so doing they will place in their libraries a volume which possesses the rare merit of being written, not only up to the present time, but, in many respects, far in advance of it—a volume which they can refer to now, and will be able to refer to in the future, with the utmost confidence and satisfaction.

X.—Thudichum's Chemical Physiology.[1]

THE need of such a manual as the one which the preface of the work before us would lead us to expect has long been felt. The author states that—

"It is a complete but concise epitome of the branch of science commonly termed physiological or animal chemistry, and will be found to contain its latest acquisitions. Any medical student who possesses the information which it contains will be enabled to meet the requirements, so far as concerns this particular subject, of any of the examining and licensing bodies in this country and abroad."

An analytical guide, forming the major part of the book, is added "for the use of those who desire to make themselves practically acquainted with the phenomena and constituents of animal bodies."

The teaching of physiological chemistry has long, we fear, been at a low ebb in this country, and it is to the continent of Europe that we have had to look for most of the chief advances in this branch of science. Perhaps as a consequence of this state of affairs our text-books on the subject have been miserably inadequate, and we have had nothing to compare with such works as have been put forth by Hoppe-Seyler, Gorcep-Besanez, and Kühne, abroad. We regret that an attempt recently made to bring out in England a translation of Kühne's valuable handbook of physiological chemistry failed from sheer lack of support. The recent addition of a course of practical physiology to the curriculum required by the London College of Surgeons has, however, given an impetus to the study of physiological chemistry in those schools where ignorance or apathy has confined the teaching coming under the head of practical physiology to mere histology. A growing need for a good and concise English text-book on the subject is therefore every day becoming more felt.

[1] *A Manual of Chemical Physiology including its Points of Contact with Pathology.* By J. L. W. THUDICHUM, M.D. London, 1872.

It is well known to chemists that Dr. Thudichum, to whom science is much indebted, was for some years engaged under the auspices of the Medical Officer of the Privy Council in an investigation into the chemical relations of disease. The results of Dr. Thudichum's labours have appeared from to time in a series of appendices to the annual reports of the Medical Officer of the Privy Council, and they were accompanied by a pretentious introduction, called a "Sketch of Chemical Physiology, and of its Points of Contact with Physiology." It is a reprint of this sketch, bound up with an analytical guide, that constitutes the work under notice.

In the preface, besides the by no means modest little account of the tenor of the first part of the work which we have already quoted, we came across the following paragraph:

"To my colleagues of the medical profession, it will afford an easy bird's-eye view of the chemical features of the field of their thoughts and actions. Its perusal will involve no unreasonable tax upon the time of any reader or student, and occasional reference to particular points is facilitated by marginal notes and a short alphabetical index."

We venture to demur to nearly every one of the propositions here laid down.

After such a flourish of trumpets the student might expect to find a carefully written digest of the science of physiological chemistry; the medical practitioner a lucid account of recent advances in medical chemistry. We think that both will be disappointed. The only consolation he will receive for his pains will be, in the words of the author, that its perusal will involve no unreasonable tax upon the time of any reader or student. But even this is doubtful. The first part of the book consists, it is true, of but forty-nine octavo pages in large type, and these by no means closely printed. But then the labour of spreading those pages! Chemical nomenclature, at all times barbarous and not easily mastered, here becomes rampant, and new physiological terms are continually manufactured. If some one were to translate Dr. Thudichum's book into readable English, it might possibly, but not otherwise, find some sort of acceptance among chemists and medical men. Our old friend sulphuretted hydrogen appears under the garb of "hydrothion;" the sugar-fermenting action of saliva upon starch is termed "ptyaline glycogeny;" gristle is "collagene tissue;" caustic soda is "sodium hydroxyde; whilst the terms "lientery," "chamolysis," and "ranculi," are used, as far as we can see, with little or no explanation. This is the more to be regretted inasmuch as the author is quite capable of using good homely Saxon English—"bacony liver," to wit, when describing amy-

loid degeneration of that organ. Surely, never was Talleyrand's celebrated remark more applicable than in this instance, that the use of language is to conceal one's thoughts. What would remain of this part of the work were it stripped of its wonderful verbiage?

One lamentable defect of Dr. Thudichum's manual is the great confusion of statements frequently met with, for which the wonderful word-coining power, in which he evidently delights, is no recompense. Thus, take his statements respecting fibrine, fibrino-plastic matter, and fibrogenous matter. Our author evidently follows A. Schmidt in his description of these, yet even by following one author he falls into manifest error. At p. 29, speaking of the stroma of the blood-corpuscles, he says that it contains a small quantity of fibrino-plastic substance, namely, paraglobuline, sometimes also termed globuline. A little further on, still quoting Schmidt, he speaks of fibrin as being formed by the combination of two constituents of the serum and corpuscles, a fibrino-plastic substance (paraglobuline) and fibrinogen. Again, at p. 48, he says "the serum has fibrino-plastic properties, *i. e.* contains paraglobuline." Thus far the teaching is distinct enough: fibrino-plastic substance and paraglobuline are synonymous expressions. We are therefore naturally enough startled at p. 56 to meet with the expression "paraglobuline *and* fibrino-plastic matter," as if two distinct substances were spoken of. The italics are, of course, our own. In speaking of the colouring matter of blood and muscle he is no less unhappy, for in two consecutive paragraphs they (hæmatocrystalline and myochrome) are by implication first stated to be different and then "identical." But the choicest specimen of dubious language occurs when brain-fat, or brain-wax, is described, which, he says:

"By Frémy, who first obtained it pure and determined its composition, was found to be an acid, and termed cerebric acid. Lately, Liebreich, having examined the same substance, gave it the name of protagon, but without any valid reason. This cerebric acid appears in crystalline needles, and gives remarkable reactions. By decomposition it yields fatty acids, glycero-phosphoric acid ($C_3H_9PO_6$) containing the whole of its phosphorus, neurine ($C_5H_{13}N$), choline ($C_5H_{15}NO_2$), and cerebrine ($C_{17}H_{33}NO_3$). This reaction makes it probable that it is proximately composed of cerebrine and lecythine ($C_{42}H_{84}NPO_9$), which latter yields the products last mentioned by the following reaction."

We omit the equation explaining the reaction, as it contains two obvious blunders (five errors occur in the eight equations given in the work). Now, if this paragraph mean anything, it is intended to express this—that brain-fat, protagon, or cerebric acid

is in some way or other a compound of two other bodies, cerebrine and lecythine; but, oddly enough, we read of "cerebric acid or lecithine;" and again, the "blood-corpuscles contain a quantity of cerebric acid or of lecithine. This has been variously called myeline (Virchow), protagon (Liebreich), and other names," so that, after all, lecithine and protagon are one and the same substance. These extracts will suffice to show with what extreme care any statement of Dr. Thudichum's must be received. In following the descriptions of other observers he appears to have confused their views. We can but consider the first part of the book as an ill-digested compilation from other authors, with a few facts noted by the author himself, expressed in grandiloquent language.

Of the second and larger part of the work under notice—the analytical guide—we can speak more favorably, though we differ from Dr. Thudichum in the opinion expressed in his preface, that this part of the treatise "is, perhaps, the most elementary that could be written for any practical purpose." On the other hand, we believe it to be unnecessarily complicated, and too much devoted to the description of obscure, and, we may add, doubtful, substances discovered, or supposed to have been discovered, by the author himself. Six pages only are devoted to a very imperfect examination of blood, whilst two are devoted to the extremely hypothetical substance which Dr. Thudichum dignifies by the term kryptophanic acid.

We fear that Dr. Thudichum's manual will do little to fill up the hiatus which our teachers of physiological and pathological chemistry lament, and that an English text-book on the subject is still a necessity.

XI.—Histories of Medicine.

THE works whose titles are given below, though differing much from each other both in plan and execution, and published

[1] 1. *Histoire des Sciences Médicales, comprenant l'Anatomie, la Physiologie, la Médecine, la Chirurgie, et les Doctrines de Pathologie Générale.* Par CH. DAREMBERG, Professeur chargé du Cours d'Histoire de la Médecine au Collége de France, Membre de l'Académie Impériale de Médecine, Bibliothécaire de la Bibliothèque Mazarine, &c. Paris, 1870. 8vo, 2 vols., pp. xxviii and 1303.

History of Medical Sciences, comprising Anatomy, Physiology, Medicine, Surgery, and the Doctrines of General Pathology. By CH. DAREMBERG, Professor charged with the Course [of lectures] on the History of Medicine at the College of France, Member of the Imperial Academy of Medicine, Librarian of the Mazarine Library, &c. Paris, 1870. 8vo, 2 vols., pp. xxviii and 1303.

2. *Faculté de Médecine de Paris. Cours sur l'Histoire de la Médecine et de la Chirurgie.* Par le Professeur CH. DAREMBERG. Leçon d'Ouverture, le 11 Novembre, 1871. Démonstration Historique de la Supériorité des Méthodes

under different circumstances, have yet so much in common that they may very well be noticed together. Each work is the substance of a course of lectures on the history of medicine, delivered before an academical audience; and this fact might well suggest a useful hint to the medical schools in this country, in all of which (so far as we know) the history of our profession is completely ignored, although it surely might claim to be considered as a branch of knowledge, which (in the well-known words of Celsus), "quamvis non faciat medicum, aptiorem tamen medicinæ reddit," (lib. i, Proem.) Dr. Dunglison's little work has been published since his death under the editorial superintendence of his son, who tells us in the preface that it is "an embodiment of the course of lectures delivered by his father at the University of Virginia many years since." It was "the desire of Thomas Jefferson, at that time Rector of the University, and of those associated with that illustrious personage in its government, that the student should learn something of the earlier progress of the science and the art, while he was at the same time pursuing a course of instruction in the usual technical details of a collegiate medical education." Accordingly Dr. Dunglison (who was, we believe, one of the five professors from England who, with two Americans, formed the whole of the educational staff of the University when it was opened in April, 1825) "was expected to teach to the best of his ability, and with due diligence," not only anatomy, surgery, materia medica, and pharmacy, but also "the history of the progress and theories of medicine." It will thus be seen that "the arduous duties devolved upon him in that institution covered a much more comprehensive field than would be possible or practicable at the present day;" and that "the labour now usually allotted to almost an entire faculty of professors was there assigned to him alone." Under these circumstances it will readily be understood that we intend to impute no blame whatever to the author when we say that his work could not be otherwise than a compilation, and that he had no time for original research. As it was written probably about five and forty years ago, it would

d'Observation et Expérimentale sur les Méthodes *à priori*. Extrait de l'Union Médicale (3e série), Novembre, 1871. 8vo, pp. 16.

Faculty of Medicine of Paris. Course [of lectures] on the History of Medicine and Surgery. By Professor CH. DAREMBERG. Opening Lecture, Nov. 11, 1871. Historical Demonstration of the Superiority of the Methods based on Observation and Experiment over *à priori* Methods. Extracted from the 'Union Médicale' (3rd series), Nov., 1871. 8vo, pp. 16.

3. *History of Medicine from the Earliest Ages to the Commencement of the Nineteenth Century.* By ROBLEY DUNGLISON, M.D., LL.D., late Professor of the Institutes of Medicine and Medical Jurisprudence in the Jefferson Medical College of Philadelphia, &c., &c. Arranged and Edited by RICHARD J. DUNGLISON, M.D. Philadelphia, 1872. 8vo, pp. xii and 287.

have been better to have published it nearer the time of its composition, when it, no doubt, fairly represented the knowledge of medical history possessed by his contemporaries. It is now published because "it is believed that it will supply the want, long felt by the profession, of a condensed history [in English] of the progress of medicine, presenting all the main facts in systematic order, avoiding, as much as possible, prolixity or unnecessary discussion of the merits of men and theories, and not laying any claim whatever to the title of an exhaustive treatise." To a certain extent it will do this, and accordingly we recommend it to our readers as probably the most useful manual they are likely to meet with. At the same time, we must add that in our opinion the editor has scarcely done justice to his father's memory in printing a work, not prepared (perhaps not even intended) for publication, without doing more to remedy some of the omissions, and modify some of the statements, which were perfectly excusable forty years ago, but which the author (if we may judge by the care bestowed on successive editions of his 'Medical Lexicon') would certainly not have allowed to continue uncorrected in the present day. If the work should reach a second edition (which may very possibly be the case), more care should be bestowed on the printing of the proper names and the titles of the works mentioned; and especially a series of references should be given to the authorities for the statements in the text. In this latter respect the work appears to disadvantage when compared with Bostock's "History of Medicine," (published in the 'Cyclopædia of Practical Medicine,' and not often met with in a separate form), which is composed partly on the same plan, and is not the result of original research, but which from the numerous references at the foot of the pages gives the reader the means of continuing his investigations on any subject mentioned in the text on which he needs additional information. The chapter on Hippocrates requires to be corrected, both as the facts of his life,[1] and the list of writings ascribed to him, by the result of the inquiries

[1] One of the errors respecting the events of the life of Hippocrates, which has been corrected in print at least five times within the last five-and-twenty years, but which reappears in Dunglison's 'History,' (p. 108), we will notice again, and recommend the editor to correct in the next edition. In January, 1849, it was stated in this Review (vol. iii, p. 210), that the mistake made originally by Sprengel in his 'History,' who said that there was no *chronological* difficulty in the way of accepting the story about Hippocrates and Perdiccas, king of Macedonia, had been repeated and perpetuated by Jourdan his French translator, by Rosenbaum his then recent editor, and by M. Littré, the editor of 'Hippocrates;' but that it had been noticed and corrected by Dr. Greenhill, in Smith's 'Dict. of Biogr.,' (art. "Hippocrates,") and in Henschel's 'Janus' (vol. iii, p. 357). Since that time the correction has been accepted and repeated by M. Littré, ('Hippocr.,' t. vii, p. xlix) and by Dr. Daremberg ('Hippocr.,' p. xxix), and it is time that Sprengel's mistake should disappear from American books also.

of Littré, Daremberg and Ermerins. The chapter on the "Medicine of the Hindoos" may be greatly improved by consulting Dr. Wise's 'History,' and also the work of Susruta, which has been published since this book was written. In fact we think that the whole work should be carefully revised, as, besides the numerous typographical errata,[1] there are several statements and sentences in it, each of which we have not time to examine now, but which, taken altogether, prevent our considering it as perfectly trustworthy.

In turning to Dr. Daremberg's works, the first thing we notice is that when he delivers his opening lecture in November, 1871, he is a Professor *at the Faculty of Medicine*, whereas, in the titlepage of the two volumes published in 1870, he appears only as a Professor *charged with a course of lectures at the College of France*. The difference is important, and bears upon a point of the history of medical education in France, of which the professor gives us an interesting sketch, and which we will briefly lay before our readers. It appears that towards the end of the last century, when the École de Santé (afterwards called the "Faculté de Médecine") was reorganized by the Committee of Public Instruction, a professorship was appointed for "legal medicine" and for the "history of medicine" (1794). Shortly afterwards (1795) the librarian of the school (Peter Sue) was appointed to lecture on "Medical Bibliography," and Thouret, the Director, on the "Doctrine of Hippocrates." In 1799 Thouret applied for the foundation of a professorship of the "Philosophy of Medicine"; but the application was not granted, and the existing professorships were in process of time amalgamated, and, finally, suppressed in 1822. In 1837 M. Dezeimeris, the librarian of the Faculty, applied for the re-establishment of the professorship of medical history, but without success; and the same result attended some other attempts made in the same direction in '45 and '59. But though it was not possible to procure the re-establishment of the professorship at the Faculty of Medicine, Dr. Daremberg was, in 1864, commissioned to deliver lectures on Medical History at the College of France; and it is the result of these lectures, delivered in the years 1864-67, that he has now given to the world in the form and with the title of a 'History of the Medical Sciences.' Thus, then, a sort of readership was

[1] Some of these mistakes, however, would seem to be due to the author, and not the printer: as, for instance, "Alexander of *Damas*," p. 172 (for *Damascus*), which is evidently taken from the French translation of Sprengel's 'History,' (tome ii, p. 100); "De Methodo Medendi *ad Glauconem*" (p. 174), where the last two words should be omitted, and where he seems to have mistaken Sprengel's reference (tome ii, p. 103);—but what is to be said for the strange word "*scientist*," used for a scientific man (p. 34)?

established, which promoted the study of medical history by employing a competent person to give his time and attention to the subject; but, the matter has at last been finally settled on a more permanent and satisfactory basis. In July, 1869, it was announced in the French medical journals that a M. de Champotran, a former "mâitre des requêtes," had left a considerable sum to the Faculty of Medicine of Paris for the purpose of founding a professorship of medical history. He had been induced to take this step by the advice of his surgeon, M. Cusco, whom in his will he recommended to the Faculty as a person perfectly capable of fulfilling the duties of the office. M. Cusco, however, declined the place, and Dr. Daremberg (probably the fittest person that could have been found) was appointed the new professor. This was shortly before the breaking out of the late war, which, of course, put a stop to medical lectures, as well as to many other things of more importance. During the siege of Paris by the Germans, Dr. Daremberg remained in the city, kept partly by his employment at the Mazarine Library, of which he is one of the librarians, and partly by his duty to his sick and wounded fellow-citizens, in which he was well seconded by his young son, who, we believe, was only a medical student at the time. Our readers will not be surprised to hear that the events of that terrible siege (the bodily fatigue, the scanty and unwholesome food, and the mental anxiety), have left their traces behind, from the effects of which it is probable that the Professor will never entirely recover.[1]

And now, having given an account of the establishment of the professorship, which is itself (as Dr. Daremberg says) "une question historique," we will turn to the work itself; and in this it seems to us that almost everything is good except the *name*, which is badly chosen. Perhaps we may explain our meaning at once when we say that if it had been called a 'Course of *Lectures* on Medical History,' we should have found it a delightful book, with which no one would have had a right to find fault; but that if it is to be called a 'History of the Medical Sciences,' there seem to us to be grave faults in the plan of the work (or rather in the disproportion of its several parts), and perhaps now and then in the execution also. In a course of lectures on any subject the Professor (unless, indeed, he intends to give a complete and systematic treatise merely cut up into *lectures* instead of *chapters*) may enjoy a certain amount of latitude in the choice of what he mentions and what he omits to notice. But surely, in a history, the writer has no such

[1] In his opening lecture, he touchingly alludes to "ma voix un peu haletante," "mon visage fatigué," "la souffrance qui me trouble"

liberty; he should exclude all extraneous matter (or at least throw it into the form of an appendix or notes), however convenient or tempting it may be to insert it; and he should not only mention whatever deserves to be mentioned, but he should also preserve a due proportion between the several parts, in accordance with their relative importance. And this is what Dr. Daremberg has not done, and therefore we repeat that if it is to be considered as a work of art, a real history, as distinguished from a series of historical sketches, it appears to us to be seriously defective. We will illustrate our position by a few instances:—The most flagrant cases of disproportion are those of Paracelsus and Van Helmont, to the former of whom the Professor allots no less than *one hundred and nine* pages, and to the latter *seventy-six*, thus occupying *one seventh* part of the whole work with these two personages. As they have received more than their due amount of commendation from some modern writers, perhaps it was necessary to represent them in their true colours, and this Dr. Daremberg has probably done more completely than any of his predecessors. The two chapters are full of interesting and (in a certain sense) original matter; but we would suggest to the Professor that, after the delivery of these lectures, the proper thing to do with them would have been to publish them as a separate *brochure*, and not to have marred the symmetry of his work by inserting them at full length therein.[1]

The same objection (though not to the same extent) may be brought against the account of Sylvius de la Boe, to whom are assigned thirty pages, and that of the iatro-mathematical (or iatro-mechanical) school, which occupies 200; but then it must be borne in mind that to this school belonged an unusual number of eminent men, *e.g.* Borelli, Bellini, Pitcairne, Boerhaave, Hoffmann, &c.

This want of symmetry, which is so great that it indicates a certain amount of want of judgment also, and which appears to us to be the chief blemish in the book, can easily be removed in a second edition; and, perhaps, if it is still to be called a *history*, those passages which belong simply to a course of *lectures* may be omitted with some economy of space, and without lessening the interest or value of the work.

The general character of the book is such as might be expected by any one who has read Dr. Daremberg's former writings, in which we find extensive, accurate, genuine learning

[1] To make the matter still more striking (if necessary) we may mention the space allotted respectively to some of the most celebrated physicians of ancient and modern times:—Hippocrates, 56 pages; Galen, 30; Harvey, 44; Sydenham, 28; Boerhaave, 16; Haller, 10; Bichat, 11; Cullen, 18.

(brought forward whenever it is required, but never without necessity, or for the mere purpose of display), enlivened by a lively (but not flippant or jocose) style, and by just and healthy sentiments. All his books are thoroughly trustworthy, and though few writings are less in need of correction than his, it adds to our confidence in his conscientious accuracy when we find him not ashamed to give a list of "Errata and Addenda." Of particular portions of the work it would be out of place here to speak at length, but we may say that Englishmen will be pleased to observe that so competent a judge does full justice to the merits of our countrymen, Harvey and Sydenham. The history of the circulation of the blood especially is a subject in which the Professor's intimate acquaintance with the opinions of the older anatomists and physiologists enables him to estimate at their true value the attempts still occasionally made to deprive Harvey of the glory of his discovery.

He gives a "Chronological Table of the Alexandrian Physicians, with a Summary of their Works," in some respects better and more complete than any previously existing, which with the accompanying "Notes Justificatives" he very properly calls an "Appendix" to Chapter VII. Another very valuable "Appendix" contains a "Study on the Different Classifications of the Periods of Medical History," with (incidentally) a critical judgment on the writers. In making this examination, the Professor has of course expressed his opinion of the different works passed in review with considerable freedom, but he does not seem to us (so far as we have observed) to have gone beyond the fair limits of criticism, or to have forgotten the excellent sentence of Mead (Preface to 'Medical Precepts and Cautions'), with which he closes his own preface, and which it would be a good thing if other medical writers (including reviewers) would more constantly bear in mind:—"The very nature of my design compelled me to take notice of the errors of other physicians; but I have been very careful throughout the work to do it with the same equity with which I would desire to have my own faults corrected." The Professor easily discovers the defects in the classifications of his predecessors, but perhaps he has not been so successful in devising one which is quite free from objections, when proposing the eight epochs (p. 25, &c.) into which he has divided his subject, and which are not in each case very distinctly marked out. The *first* he calls "theurgic or empiric" (perhaps not very well chosen names), by which he appears to mean the primitive medicine in the hands of priests, &c., and such as is shown in the hymns of the Hindoos in the 'Rig Véda.' The *second* and *third* he joins together (p. 79), and they carry us to Hippocrates (inclusive); the *fourth* compre-

hends the Alexandrian school, with Herophilus, Erasistratus, &c.; the *fifth* includes Galen; the *sixth* extends to the fifteenth century; the *seventh* ends with the publication of the first edition of Harvey's work, 'De Motu Cordis et Sanguinis,' in 1628; and the *eighth* and last continues at the present time. This bare enumeration of the Professor's eight epochs, though without giving the reasons for his classification, may be enough to let our readers see that the classificatiou may (like most similar arrangements) be considered as more or less arbitrary, and that the number of the classes might be either increased or diminished with equal plausibility.

In taking leave of the Professor, we beg to offer him our best thanks for his most valuable and interesting book, our sincere congratulations on the well merited professorship, and our hearty good wishes that he may soon be so far restored to health and strength as to be fit to resume the duties of his office, which probably no one in Europe would be able to perform so efficiently as himself.

XII.—Fayrer's Poisonous Serpents of India.[1]

THE subject of snake-poisoning has recently attracted very considerable attention. Much has been drawn to it by the observations made by Professor Halford, of the Melbourne University, on the effects of the injection of liquor ammoniæ in poisoning by the Australian snakes, and on the microscopic changes which he believed he observed in the blood of animals killed by their bite. The costly and magnificent work before us, although it in no respect confirms Professor Halford's observations, at least, in as far as they were expected to apply to the Indian snakes, is a very valuable fruit of the attention which has been called to the subject by the writings of the Melbourne Professor. In it Professor Fayrer has given us, first, a good zoological account of the Indian *Thanatophidia*, in the descriptions and definitions following Günther and other authors of repute, but supplementing their accounts with many original facts and observations of great interest; secondly, some valuable statistical information proving the great amount of mortality from snake-bite in the State of Bengal; thirdly, a treatise on the treatment of snake-bite based on the records of many

[1] *The Thanatophidia of India, being a Description of the Venomous Snakes of the Indian Peninsula, with an Account of the Influence of their Poison on Life; and a Series of Experiments.* By J. FAYRER, M.D., C.SI., F.R.S.E. Fol., p. 156. London, 1872.

cases and careful experiments; fourthly, a large collection of cases of snake-bite from the reports of civil and military medical officers; and, lastly, twenty-nine series of experiments; most of them conducted by himself, and a few by Mr. Richards, of Bancoorah, and Mr. Stewart, of Poorie; made with various Indian species of poisonous snakes upon all sorts of animals, which form, we think, one of the most remarkable investigations upon a subject of the highest scientific and practical importance ever made in any age or country. With some of the results of his observations we shall now endeavour to make our readers acquainted.

The first section of the work is occupied with anatomical and zoological descriptions. The most important and dreaded of the Indian snakes is the cobra, *Naja tripudians*. Dr. Fayrer gives us some very valuable information respecting the varieties of cobra, which are numerous, and which have been previously overlooked by writers on Indian ophiology. The natives are familiar with these varieties, which are distinguished by native names derived generally from their colour. The great distinction made by the snake-catchers of Bengal is between the cobras with the spectacles on the hood, or "gokurrahs," and those with one ocellus or other mark on the hood, named "keautiahs." These the natives regard as distinct from each other, as the krait or *Bungarus cæruleus* is from the sankni or *Bungarus fasciatus*.

"The gokurrah, they say, is essentially a snake of the town or city. The keautiah is of the fields and jungle. The gokurrah is slower to kill, as its poison is thicker though most deadly. The Keautiah's poison is thinner, and takes effect sooner, though it is not more fatal than that of the gokurrah. Both, they say, incubate, and the snake-man informs me that, over and over again, he has dug them out of holes sitting on their eggs. The gokurrah takes to the water reluctantly; the keautiah freely, and will remain for a considerable period under water. The hood of the keautiah is smaller relatively than that of the gokurrah, and the body is more attenuated; it is more slender and active than the gokurrah. The varieties of both eat about every sixth day; they deposit their eggs once in the year, and that in the rainy season. The keautiah is often found during the rainy season in the huts of the villages, where it has been driven to take shelter by inundation. It is as unusual to find a keautiah, though, in the ruins or débris of an old building, as it is to find a gokurrah in the open country. The snake-catchers here say that they believe that whereas the gokurrah is found all over Hindostan, the keautiah is, if not confined to Bengal, rare in the north-west, and other parts of India. This, however, is by no means certain, and requires confirmation." P. 7.

Amongst the magnificent coloured plates, from drawings done by native artists in the Government School of Art which alone would make the work of great value to the naturalist, are delineations of ten of the native varieties of cobra.

Another hooded snake belonging to the same family as the cobra, but generically distinct, is the *Ophiophagus elaps* or hamadryad, the sunkerchor of Bengal. It is, perhaps, the largest venomous snake, growing to the length of twelve or fourteen feet, and is very powerful and aggressive. Its poison is terribly potent, although not more potent, judging from Dr. Fayrer's experiments, than that of the cobra. Its pre-eminence in size, amongst Thanatophidia, is disputed by the bushranger, or curucucu, of British Guiana, a viperine snake which grows to fourteen feet. The ophiophagus, as its name denotes, lives on snakes. The snake-catchers occasionally have them, but they are difficult and dangerous to handle before their fangs are removed. Dr. Fayrer gives an account of several living specimens with which he performed his experiments. The first one was brought to him by some snake-men, but its fangs had long been extracted. He describes it as very much under the control of the snake-man who exhibited it. It would sit up, erecting its hood, and following the motions of the hand like a cobra.

"On two different occasions it ate snakes in my presence, two specimens of *Passerita mycterizans*, that had been killed by a cobra. The snake-man put the head of the *Passerita* into the hamadryad's mouth, and in about a quarter of an hour it gradually swallowed it. During the process it moved slowly about with the head, neck, and hood dilated, and it looked very odd with the smaller snake hanging out of its mouth. The fangs had been, as I have said, extracted, but on pressing the poison gland a deep yellow-coloured viscid fluid exuded. I collected a few drops of this and inoculated a drop of it into the thigh of a fowl; the bird sickened and died in about three hours." Pp. 9-10.

The hamadryad is terribly aggressive. If disturbed it will give chase and pursue over hill and dale. Dr. Mason, in his work on Burmah, gives an account of a man who was pursued by one of these serpents which swam across a river after him. He luckily bethought himself of his turban, and in a moment dashed it upon the serpent, which darted upon it like lightning, and for some moments wreaked its vengeance in furious bites; after which it returned quietly to its former haunts.

The krait or *Bungarus cæruleus* is a snake which is deservedly dreaded in India. It is common, and is, next to the cobra, most destructive to human life. It is more fatal than its congener and relative the *Bungarus fasciatus* or sankni. Like the cobra it often gets into buildings and insinuates itself into

book-cases, cupboards, &c. Dr. Fayrer knew of an instance, where, after a night's dâk in a palanquin, a lady, in taking out her things on arriving at her destination, found a krait coiled up under her pillow; it had been her travelling companion all night.

Two Indian vipers, Russell's viper, *Daboia russellii*, and the kuppur, *Echis carinata*, claim attention. The daboia is frightfully poisonous. Dr. Fayrer thinks it nearly as poisonous as the cobra. He had one forty-four inches in length which lived in captivity a whole year without food or water; it obstinately refused both, but was vigorous and venomous to the last. It died suddenly. One of these snakes will kill a fowl in from thirty-five seconds to several minutes, a dog in from seven minutes to several hours, a horse in eleven hours. These are less rapid results than those produced by the cobra, but the poison is just as fatal. Dr. Fayrer relates the case of a gentleman who took one of these snakes home, thinking he had a young boa. He was undeceived by the daboia killing a dog which came near it.

The most active and aggressive little venomous snake is the *Echis carinata*. This creature is always on the defensive, always ready to attack. The echis does not hiss, but it makes a sound very like hissing by rubbing its carinated scales against each other. Dr. Fayrer received a batch of seven of them from Delhi—

"They laid all knotted and coiled together in a corner of the cage, so close that it was impossible to distinguish one from the other, a head peeping out here and there. When disturbed they immediately separated, and coiled themselves up in a series of convolutions with the head erect, the mouth open, the eye looking particularly vicious, and with a loud hissing sound they prepared to strike, and frequently they darted the whole body forward a foot or more in the attempt to strike. The head was always erect, and the whole aspect and attitude was that of intense malice and mischief."

The hissing sound they make is by the attrition of the carinated scales; the snake without changing place is in constant motion, like the vermicular movement of the intestine.

Dr. Fayrer's account of the *Hydrophidæ* or poisonous sea-snakes is of great interest. Several new species described by Dr. Anderson, of the Indian Museum, are for the first time given to the scientific world in this work. The fangs of the *Hydrophidæ* are very small, and the bite they inflict is almost imperceptible, but it is most deadly. Persons who have been bitten by them when bathing usually die in from two to twenty-four hours.

We must refer our readers to the work itself for accounts of the

other poisonous species and genera, *e.g.* Xenurelaps, Callophis, Trimeresurus, Peltopelor, Hypnale, &c., and pass on to the second the section, which treats of the fatality from snake-bite in the presidency of Bengal. This appears to be frightfully high. In the year 1869 there were upwards of 11,000 known and reported cases, and the number of unreported cases was probably not much less. A liberal system of rewards for dead snakes would undoubtedly lessen the evil.

If Dr. Fayrer's work had done nothing else than show the utter inutility of all the so-called antidotes, whether of native or European origin, against the bite of the more poisonous of the Indian serpents, he would have rendered good service. Inutility is, in truth, not a sufficiently strong word to use with regard to them, for reliance upon them has often bereft the unfortunate victim of his only chance by consuming the precious time which should have been employed in the use of the ligature excision and cautery. Ammonia, aristolochia, arsenic, Tanjore pills, eau de luce, liquor potassæ, numerous native remedies, snake-stones, the injection of ammonia, potassa, Condy's Fluid, have all been tried, and were all found equally unavailing when an animal had been fairly bitten by a vigorous cobra. But both cases and experiments confirm the value of the ligature when tightly and promptly applied above the bite, and followed by speedy excision and the application of the actual cautery, or of strong nitric acid; or, better still, in the case of a finger or toe, by immediate amputation at the next joint above. The following is Dr. Fayrer's summary of all that he thinks can be done:

"I would summarise the treatment of snake-bite as follows:—Apply at once a ligature or ligatures at intervals of a few inches, as tight as you can possibly tie them, and tighten the one nearest to the wound by twisting it with a stick or other such agent. Scarify the wound and let it bleed freely. Apply either a hot iron or live coal, or explode some gunpowder on the part, or apply either carbolic or some mineral acid or caustic. Let the patient suck the wound whilst you are getting the cautery ready, or if any one else will run the risk let him do it. If the bite be on a toe or finger, especially if the snake has been recognised as a deadly one, either completely excise or immediately amputate at the next joint. If the bite be on another part, where a ligature cannot be applied, or, indeed, if it be on the limbs above the toes or fingers, cut the part out at once completely. Let the patient be quiet. Do not fatigue him by exertion. When or even before symptoms of poisoning make their appearance give eau de luce, or liquor ammoniæ, or carbonate of ammonia; or, even better than these, hot spirits and water. There is no occasion to intoxicate the person, but give it freely, and at frequent intervals. If he become low, apply sinapisms and hot bottles, galvanism,

or electro-magnetism over the heart and diaphragm. Cold douches may also be useful."

We must pass over the collection of cases of snake-bite which form the fourth section of the work, in order to glance at some of the experiments which have led Dr. Fayrer to his results. Before doing so, however, it will be interesting to non-Indian readers to learn how the cobras and other poisonous snakes used in the experiments were manipulated. The professional snake-men handle these snakes with remarkable ease, and make them inflict a bite upon any substance or animal, or shed their poison as they please:

"The mode of procuring the poison is very simple; it is obtained by making the cobra bite through a thin leaf stretched across a mussel-shell; the poison, like limpid syrup, runs in considerable quantities down the grooved tooth into the shell, where it is collected, and may be preserved for experiment."

A full-grown cobra will yield half a drachm. Five drops injected into the venous circulation of a large dog will kill it in two minutes and a half. Four drops inoculated in the muscular part of the thigh of a fowl killed it in four seconds.

The cobra, until roused, is sluggish, and its muscular power is not sufficient to enable it to turn upon any one who is bold enough to take it by the tail, hold it at arm's length, and prevent it darting at his legs by pressing the head to the ground with a stick held in the other hand. Thus it is the snake-catchers manage them. Dr. Fayrer writes:

"In the present instance there were three full-grown fierce cobras confined in a small box. Raising the lid carefully, the snake-catcher put in the end of the stick, and lifted out one about four feet long, hissing, and with its head erect, looking the very incarnation of mischief. He then gently dropped the snake on the ground, and as soon as it began to move off seized it by the tail, raised it off the ground, and placed the stick, holding it at arm's length, about midway under its body. He then allowed it to struggle and to make efforts to dart at him over the stick, on which it hung in a festoon, keeping up an oscillating motion with his knee, which seemed to influence the reptile's movements, as it kept time, moving its head at about the same rate as the man moved his knee. He then placed it on the ground, dragging it gently by the tail; and, watching his opportunity, placed the stick on the snake's back just behind the head, and so pressed it to the ground. Holding the tail under his naked foot, he quietly seized the snake behind the head, and squeezing it, made it open its jaws, when the poison fangs could be distinctly seen; in this way the snake was made to bite the animals experimented on, or the leaf, when the poison was collected in a shell."

All living creatures, except the poisonous snakes themselves,

seem to succumb to their venom. Mammals, birds, fish, reptiles, and invertebrata were made the subjects of Dr. Fayrer's experiments. The mongoose or ichneumon has been supposed to be an exception. But it is not really so. The mongoose will attack, fight, and probably kill the cobra, but if it is fairly bitten it dies. In the experiments made, the mongoose and cobra reciprocally bit and scratched with their teeth until both were bleeding; but when the mongoose was taken out of the cage and the cobra was made to close its jaws upon the mongoose's thigh, it soon died. Cobras cannot poison each other. Cobras and daboias will bite each other, and on many occasions not seem the worse for it. The same is true of the krait, but not invariably. Some of the experiments seem to show that a cobra or daboia may poison a krait, or *vice versâ*, but that they escape more frequently than they suffer. The *Bungarus fasciatus* is affected by the cobra-poison, and innocent snakes die rapidly. The warm-blooded animals, and especially birds, die most quickly. A fowl will be killed by a vigorous snake in a few seconds. The rapidity of death is generally proportionate to the size of the animal, but not invariably so. For instance, a small cat will resist the action of the poison longer than a dog double its size.

The matters of most professional interest in the book are the investigations of the action of liquor ammoniæ, liquor potassæ, Condy's Fluid, quinine, &c., when injected into the blood, and the influence of the poison on the blood itself. With regard to the value of liquor ammoniæ when injected subcutaneously, or into the blood itself, Dr. Fayrer and Mr. Richards, of Bancoorah, have made so many varied and most careful experiments that the question as regards the Indian snakes may, we think, be said to be set at rest. Out of a number of others, we select three as supporting this conclusion.

Experiment 11, 13th series:

"The external jugular vein of a dog was exposed at 3.4 p.m., and four drops of cobra poison were injected; at least, one drop was lost, the other three entered the vein. 3.10.—Dog looks dejected, and ears drooping; he lies down. 3.33.—Beyond being very sluggish, no symptoms of poisoning. 3.46.—Very sluggish; lies down. 3.47.—Liquor ammoniæ, sp. gr. ·959, sixty drops injected into jugular vein; dog lies quiet; heart beating rapidly; respiration very feeble. 3.54.—Heart action very rapid; breathing rapid; muscular twitchings. 3.57.—Injected sixty more drops into the vein; muscular twitchings continue. 3.59.—Dead."

Dr. Fayrer remarks:—"Poison injected at 3.4, death at 3.59: in fifty-five minutes. The quantity of poison was very

small from a weakened snake. No effect was produced by the ammonia."

Experiment by Mr. Richards, p. 94:

"A country goat was bitten by a cobra that had been in my possession some time, at 12.55 p.m. At 1 p.m. sixty drops of undiluted liquor ammoniæ were injected into the left jugular vein. The animal on being released limped off, but did not seem affected by the bite. 1.15.—Seems slightly sluggish. 1.30.—In the same state; abdomen tympanitic. 1.40.—In the same drowsy stupid state, but is standing up 1.47.—Suddenly fell over, and became convulsed 1.50.—Dead in 55 minutes."

The third experiment we shall quote is a crucial one. It is No. 17 of the 13th Series:

"A large and powerful dog had the right external jugular vein exposed. Twenty drops of a mixture of fresh cobra poison, taken from the snake the same day, one part, and liquor ammoniæ sp. gr. ·959, forty drops or two parts, was then injected with the hypodermic syringe into the vein. The time of the insertion of the fluid was 4h. 27m. 30s. The effect was instantaneous; the dog struggled, howled, and was convulsed on the table: he was immediately released and placed on the ground, but was already almost unconscious and convulsed. He made an effort to rise on his legs and fell prone on his belly. Within one minute respiration had ceased, though the heart's action continued faintly. This ceased, and at 4.30 a.m. he was quite dead. The action of the poison with the ammonia was frightfully rapid in this case. Death occurred in two minutes and a half, complete unconsciousness within a minute; and only by the faint beating of the heart which continued for two minutes and a half, was any sign of life manifested. This, surely, is fatal to the theory of ammonia injected into the circulation being of any benefit in snake-poisoning. In this case the poison and the so-called antidote were injected synchronously; the result was almost instant death. The experiment was performed by Dr. Ewart and myself with the greatest care, and certainly no air entered the vein."

The experiments with liquor potassæ, Condy's Fluid, &c., were equally decisive as to the utter inutility of these substances when a real bite by one of the more venomous snakes has been inflicted. One of the most practical results shown by Dr. Fayrer's experiments is the remarkable effect of carbolic acid on these creatures. Carbolic acid is useless as an antidote against their poison, but it is a most potent poison to the snakes themselves. A drop or two put into a cobra's mouth will kill it in a few minutes, and the snakes show the greatest repugnance to it. Dr. Fayrer's experiments clearly suggest, as he says, the value of a free application of carbolic acid or perhaps even of coal-tar to walls and timbers and apertures of houses in India. Carbolic acid would no doubt keep the snakes at a distance.

Dr. Fayrer's work contains many observations on the blood in snake poisoning. He finds that in cases of poisoning by the colubrine snakes the blood of the animals experimented on nearly always coagulated firmly; but this was not the case in poisoning by the viperine snakes, the blood generally remained fluid. This result, however, was not invariable. Numerous observations failed to confirm Professor Halford's descriptions of corpuscular changes observed after poisoning by the Australian snakes.

The blood of an animal poisoned by a snake bite if injected into another will kill it, but the flesh of fowls and pigeons killed by poisonous snakes may be cooked and eaten with impunity. The poison may be absorbed through the conjunctiva, and through serous and mucous membranes, including the gastric. The practice of sucking a snake bite is therefore not absolutely safe. This fact alone is of the greatest importance.

There are some valuable experiments by Dr. H. E. Armstrong and Dr. Brunton on some cobra poison sent to this country by Dr. Fayrer, which are published in the Appendix. Dr. Armstrong examined the poison chemically, and found it contained a peculiar *albuminoid* substance; and Dr. Brunton found the alcoholic extract of the poison as fatal to animal life as the poison itself.

Here we must stop. Dr. Fayrer's work is a monument of courage, sagacity, ingenuity, and public spirit. The experiments, of which there are twenty-nine, were undertaken and conducted throughout at his own expense, and must have made enormous inroads upon his time and energy. The Indian Government, recognising the great value of his work, undertook the expense of publication, and the book appears in a sumptuous form. Thirty-one plates illustrate it, of which twenty-eight are coloured representations of the snakes, and the others are lithographs of Dr. Fayrer's dissections and representations of the mode in which the snakes are handled by the snake-catchers. These which are all taken from drawings made by native artists, bear stong testimony to the progress the Hindoos are capable of making in the fine arts. Since the great work of Russell no such valuable contribution to our knowledge of these formidable reptiles has appeared, and we trust that the information which the distribution of Dr. Fayrer's book throughout India must diffuse will speedily lead to their extermination, and the consequent annual saving of thousands of lives.

Bibliographical Record.

Bastian on the Beginnings of Life.[1]—In our April number we noticed the small work of Dr. Bastian, which acted as the precursor of the two large volumes now before us. As our space precludes us from the analysis of the work in detail, we shall only call attention to the two principal lines of argument adopted by Dr. Bastian, and adduce experiments on which he considers he has established his hypothesis, which, as our readers will recollect, was nearly identical with that of the "spontaneous" or "heterogenetic" origin of life on this planet.

The argument that "dead matter" (using this phrase in the sense employed by Dr. Lionel Beale) is capable of producing even from the centre of crystals of neutral ammonic tartrate, and in solutions containing ammonic silicate certain spores and filaments which have a considerable resemblance to those of true organic matter is developed at length. These organisms

"Have been either fungus filaments and spores similar to those represented in Figs. 29 and 36; *Torulæ*, such as have been sketched in Fig. 28; various kinds of flagon-shaped bodies of a light brown colour (apparently budding out into filaments, and containing blocks of protoplasm within), or else roundish spores of very variable shape and size—some being smooth externally, others rough, and most of them having thick walls."

In addition to these unmistakable organisms (which have, in some cases, been proved to be truly living) obtained from the saline solutions, other bodies have been encountered, whose real nature is deemed to be very doubtful. It may appear strange to some of our readers, that Dr. Bastian, who certainly has a desire to find life or living tissues in every imaginable spot, and under every variety of circumstances, has devoted a large portion of his space to argue, in spite of the opinion of our highest authority on fungi—the Rev. M. J. Berkeley—to prove that *sarcina* is not a real living organism. This argument rests upon the facts that *sarcina* has only been seen to undergo a process of growth and development, and never one of spontaneous fission, without which it can have no strict claim to be considered as one of

[1] *The Beginnings of Life; being some Account of the Nature, Modes of Origin and Transformations of Lower Organisms.* By H. CHARLTON BASTIAN, M.A., M.D., F.R.S. Two volumes. London: 1872.

the lowest kind of living things; that *sarcina* met with in ammonic tartrate solutions like that from the stomach has always existed amongst the sedimentary deposits; and that in other ammonic tartrate solutions, in which *sarcina* has either been absent, or in minute quantity, a sedimentary matter has been found, having a very strong general resemblance to *sarcina*, though the appearance of this has been such as to lead to the belief that it is a kind of modified crystal rather than a living substance; and this *sarcina*, even when obtained from the human stomach, varies considerably as regards its ultimate pattern or arrangement.

Dr. Bastian is, however, not content with demonstrating a change in the particles of certain crystals, but also alleges that an undoubted vegetable growth (*Vaucheria*) of which the structure had become partially disintegrated, and the spores decolourised, some of the chlorophyle-corpuscles being colourless, and others still green, a conversion takes place in a mass, containing fatty looking particles and globules; which animalised mass becomes in time finely granular and somewhat smaller, next becomes segmental; is resolved into a worm-like embryo which exhibits very slow movements; and at last appears in a form well known as *Diplogaster* of Schultze, which in course of time lays eggs and propagates its kind like other nematoid worms.

The fact that animals

"With such distinct and specific organs, and of different 'sexes,' too, should arise in this definite manner from the reproductive products of a plant, will, doubtless, seem to many to flavour more of fable than of fact. After the observations which have been detailed, however, we must accept the occurrence of such phenomena as established facts—just as we are compelled, and are now quite accustomed, unhesitatingly to believe in the reality of other equally inexplicable phenomena. When we are able really to explain the reason of the processes by which one minute vesicular mass of fatty and albuminoid particles develops into a man, another into a fish, and another into an insect, we may then, with a little more show of reason, think of rejecting other more or less similar facts because they are incomprehensible."

Arguments of this kind form a very large proportion of those advanced in Dr. Bastian's work. His plea for the origin of life, by what he terms *archebiosis*, deserves, however, the most careful examination, and, as we have before said, it is only by the direct disproof of his facts that it can be controverted. The arguments of Dr. Bastian and the facts must be considered as two entirely different subjects of investigation.

It is hardly necessary to say that in the main Dr. Bastian agrees with the views of Dr. Grant as to the complex nature of the organisation in the higher animals, that *e. g.* the leucocytes, or "white

corpuscles" of the blood are practically the same as young *amœbæ*, and capable of separate existence. Their death only occurs when the totality of the organism which supplies them with *pabulum* becomes incapable of nutrifying them. Such views, while they have the peculiarity of representing the teachings of a minority, are supported by a series of minute experiments. We shall take only one of these, and one likewise of the most remarkable.

In the brain of a patient who had died of rheumatic fever at University College Hospital, and in whom an exceedingly high temperature had existed for a few hours before death, there were observed, when the arachnoid was cut through, a large number of most actively moving particles.

"Many of these were mere spherical particles of various sizes, but others were distinct and large *Bacteria* made up of two almost cellular segments; and every portion of the pia mater that was examined showed similar moving particles and *Bacteria*. The brain was then covered with a bell-glass, and when portions of the pia mater—again taken from between previously unseparated convolutions—were examined after an interval of twenty-four hours, the large *Bacteria* had, considerably, increased in number, whilst the small spheroidal particles seemed to be as plentiful as before. When portions of brain substance from some central parts of the organ were also examined at this time, moving particles and *Bacteria* were seen to exist in the greatest abundance amongst the disintegrated nerve elements, which had probably been poured out from the blood-vessels."

From this observation Dr. Bastian proceds to contend that these *Bacteria* must have existed in the blood of these vessels before the brain had been removed and before the skull was opened. *Bacteria* are not produced in any fluids under two or three hours.

"Their origin could not, therefore, have been due to *Bacteria* germs derived from the atmosphere, which, on removal of the skull-cap, had in some mysterious ways insinuated themselves into the blood-vessels. They must either have existed in the blood during life, or else they must have been produced *de novo* in this fluid after death. There is strong reason for believing that *Bacteria* existed in the blood during the life of the individual."

He, therefore, concludes that these objects were newly evolved by reason of changes taking place in the blood after death, in or near the situation in which they were found. He imagines that in all probability "*if examination had been made*" they would have been found disseminated throughout all other parts of the body, just as they were actually found in different portions of the brain. An inquirer might ask what reason could exist why further examination was not made to verify this important statement. The blood does not contain *Bacteria* during the life of the individual.

The researches of Dr. Burdon Sanderson ("fourteenth Report of Medical Officer of the Privy Council) seem to show that the blood and other parts of the body do not generally exhibit any zymotic tendencies. The following argument is adduced against the researches of Dr. Burdon Sanderson:

"There is a great tendency with others to conclude that *Bacteria* do not arise *de novo*, because there is no evidence of such occurrence when dealing with Pasteur's solution or a few other fluids, different from those in which the process is said to occur. Let any person, for example, repeat Dr. Sanderson's *thirteenth* [query third?] experiment ('Thirteenth Report of the Medical Officer of the Privy Council), with a strong infusion of hay or turnip, rather than with Pasteur's fluid, and then such results will occur that, from Dr. Sanderson's data, he will have no option but to admit that *Bacteria* do arise *de novo*. It is surprising that such an experiment was not tried in the face of all that has been said concerning the productivity of such fluids. The real laws by which contagion is regulated can never be adequately understood, unless one knows whether the *contagia* with which one is concerned can, under any circumstances, arise *de novo*. This seems to me to be the point which should be first ascertained."

We have now glanced at some of the principal arguments contained in Dr. Bastian's work. We are confident that neither the medical profession nor that section of the scientific and often non-scientific public which occupies itself with what are called physiological questions, will accept Dr. Bastian's statements without much careful examination. It is necessary that all his experiments, amounting to more than a thousand, should be repeated by some one who takes no share in the polemics of the day. It is also necessary that, before these experiments are rejected, some counter facts shall be conclusively proved which will satisfactorily demonstrate the truth of the assumptions held by Schleiden, Schwann, and the last generation of physiological teachers.

The Martyrdom of Man.[1]—To review a work which passes over such an enormous number of distinct subjects as does that of Mr. Winwood Reade, and which in part trenches on the domain of the student of ancient history and of the theological reader, would in these pages be an impossible task. We have to look upon the author chiefly as an anatomist, as an anthropologist, as a zoologist, and as an antiquary; and if the examination of his pages leads us to express some doubt as to the nature and value of the facts he considers himself to understand, we may leave to other hands the task of examining his qualifications to write down the metaphysic or the theology of the majority of our educated countrymen. His chief

[1] *The Martyrdom of Man.* By WINWOOD READE. 8vo. London: 1872.

guides in science (page v) have been "Darwin, Lyell, Herbert Spencer, Huxley, Tyndall, 'Vestiges of Creation,' Wallace, Tylor, and Lubbock." From which of these authorities of variable weight some of the following statements are derived is a subject which will amuse the younger members of our families on winter evenings. On page 391 it is stated—

"At last the hour of birth approaches; coiled within the dark womb he sits, the image of an ape; a caricature and a prophecy of the man that is to be. He is born, and for some time he walks only on all fours; he utters only inarticulate sounds; and even in his boyhood his fondness for climbing trees would seem to be a relic of the old arboreal life. Since, therefore, every man has been himself in such a state that the most experienced observer could not with the aid of the best microscopes have declared whether he was going to be man or plant, man or animalcule, man or mollusc, man or lobster, man or fish, man or reptile, man or bird, man or quadruped, man or monkey; why should it appear strange that the whole race has also had its animalcule and its reptile days? But, whether it appears strange or not, the public must endeavour to accustom its mind to the fact which is now firmly established, and will never be overthrown."

A more accurate and less imaginative anatomy would have, however, pointed to the early period of fœtal life at which the embryonic hind-limb, for example, of the monkey affords a manifest distinction from the similar characters in the human hand. Owen[1] has put this point very clearly in his great work on comparative anatomy. If Mr. Winwood Reade, instead of taking as his guides to science some of the third-rate compilations he has cited, had consulted G. Breschet ('Recherches sur la gestation des Quadrumanes,' 4to, 1845), he would have learnt that

"The inner digit of the pelvic limb in the fœtal monkey (*Cercopithecus sabæus*) already shows by its relative shortness and divergence from the others that it is destined to oppose them, and to terminate the member by a prehensile *hand*: while from the earliest manifestation of the digits of the same limb in the human embryo the 'hallux,' by its proportions and parallelism with the other toes, indicates the destination of the answerable part to become a plantigrade *foot*, perfected to sustain and move the body of an erect biped."

When, however, Mr. Reade turns from what he has read to what he has heard, he becomes a still less reliable scientific authority. On page 419 he says:—"The chimpanzee is not so large a creature, or so strong as the gorilla; but, as I was informed by the natives in that country where the two species exist together, the chimpanzee is the more intelligent of the two."

[1] Anatomy and Physiology of Vertebrates, iii, 754.

Whether such a statement can be accepted by those who have dissected the brain of the chimpanzee, or who have examined the partial dissection made by Owen[1] of the brain of the gorilla, we cannot imagine. The enormous cerebral superiority of the gorilla over the chimpanzee has once for all been clearly demonstrated, and we are sorry to see that Mr. Reade, since the publication of his work 'Savage Africa,' has become imbued with a spirit of veneration for the veracity of the West African negro, on a question involving some metaphysics. Possibly Mr. Reade will find out that the cunning negro, if indeed he really was intelligible to the traveller, hoaxed him, when he described the "tschego" as being more intelligent than the less known "engena."

Whether as an anthropologist, or as an antiquary, Mr. Reade is alike unfortunate in such a passage as the following (p. 468):

"A people, apparently of Tartar origin, had already streamed into their continent [Europe] from Asia; but the true aborigines were not extinct; they inhabited huts built on piles in the lakes of Switzerland; they herded together in mountain caves. They were armed only with stone [?] weapons; but they cultivated certain kinds of grain, and had tamed the reindeer, the ox, the boar, and the dog."

That the *pfahlbauten* of Switzerland belong to a period coeval with that of the dwellers in the caves of Belgium, or even of Aquitaine, is a statement hazardous enough; but we cannot follow Mr. Reade when he asserts that the builders of the lake habitations were the aborigines of Europe. That bronze implements and Roman coins were used by our aboriginal population is a theory which demands an examination in the pages of other than a medical periodical.

Zoologically, Mr. Reade is often very vague. What are we to make of a passage like the following applied to an Arabian horse? (p. 20):—"With tail raised and spreading to the wind, with neck beautifully arched, mane flapping, red nostrils dilating, and glaring eyes, she rushed like an arrow into the midst of the melée."

We were always under the impression that the principal defect of which connoisseurs complain in the Arab horses is the fact that they are generally what is termed "ewe-necked," a blemish which shows in the horses of Arab descent which have populated the New World. The whole work abounds with statements of this calibre. We had looked upon Mr. Reade as an energetic young African traveller who had devoted his life to the furtherance of our knowledge of the "dark continent," on which there exists a wide field for discovery in medicine and general science. We are sorry that instead of occupying his pen with a useful record of travels he

[1] Owen on the Gorilla, p. 39.

has published a work replete with scientific error, and containing passages which must surprise all students of natural science. We trust the future martyrdom of man will not consist in having to read through many works like the present. The work is got up by the binder in a precise imitation of Mr. Darwin's 'Origin of Man,' but the contents are far different.

Melia's Origin of Man.[1]—As a *pendant* to the work of Mr. St. George Mivart on the genesis of species, the present unpretending little volume will probably outlast in reputation many greater and more celebrated works. The object of the author is to call attention to the numerous theories of the origin of man proposed by the ancient classical philosophers, and to contrast them with the modern hypotheses on the subject. Sustaining, as Dr. Melia does, on grounds which appear to us to be necessary to the maintenence of his position, the theory of the descent of mankind from one stock, he is inclined to suppose for the commencement of the stock the operation of some law which has not come into action subsequently, or as he well puts it—

"Though it be admitted, for the sake of argument, that the body of the first man and woman, our progenitors through natural metamorphosis, or transformations, were descended from some lower form of animals, through insensible and indefinite gradations, going back to a first living monad, yet the conclusion above stated is not weakened, it has the same mighty application. For the supposed first monad, the origin of the long series of vertebrata (from which, in the hypothesis illustrated by Darwin were all supposed to come forth at last) must have had its origin from some other efficient cause extraneous to the series of the said transformations. This efficient cause, if it be supposed to be derived from some other previous being or beings, must lead us at last to a first cause or efficient principle, out of the series and independent of it, in order to avoid the absurdity of admitting that there are effects without their cause."

Though Dr. Melia had the disadvantage of publishing his work before Mr. Charlesworth's discovery of perforated shark's teeth, indicating human action in the Pliocene Red Crag of Suffolk, yet he points out that the question of the extreme antiquity of man may be discussed without attempting to prove a greater antiquity for the Shemitic race than the short period of time during which its traditions have been preserved to us. With regard to the mental differences between man and the inferior animals, Dr. Melia agrees with the modern school of philosophy as to the non-existence of innate ideas in young children, and cites a series of most interesting facts in illustration as to deaf-mutes, from L'Epeè, Lieaud, Saloan, Poulmière,

[1] *Hints and Facts on the Origin of Man, and on his Intellectual Faculties.* By Pius Melia, DD. London: 1872.

Berthier, Goudelin and others, to show that where social instruction is wanting the faculties of speech and reason never obtain their development.

As a contribution to the metaphysical portion of the question relating to the origin of man, Dr. Melia's work, which is written in a singularly elegant and liberal spirit, may be perused with profit by those who are ranged on either side of the controversy relating to the genesis of the human species. It contains a remarkably clear *précis* of the palæontological and anthropological arguments, whilst it brings out the philosophical and ethical part of the subject into prominent relief.

Lectures on Aural Catarrh or the Commonest Forms of Deafness and their Cure.[1]—In reading publications which profess on their title pages to *cure* anything, and especially when the subject of cure happens to be deafness, impaired sight, cough, or any other unpleasant symptom of disease; however good may be the contents of the treatise, it is difficult at first to keep in view the fact that the book is intended for the use of the profession to the complete exclusion of the public, and especially so if it should happen, as in the present instance, that in the first few pages impaired hearing is spoken of as "one of the most severe afflictions incident to mankind."

It is therefore a matter of regret that in the little book before us Dr. P. Allen should have thought it necessary to add to the title of "Lectures on Aural Catarrh," "the Commonest Forms of Deafness and their Cure."

Twelve short lectures are devoted to the diagnosis and treatment of non-purulent and purulent catarrh of the middle ear, and include a very good description of the anatomy of the tympanum. Without pretending to be an exhaustive account of the various effects which are produced by affections of the lining membrane of the middle ear, the methods of examining the ear, the value of appearances in diagnosis, and symptoms are described in such a way as to be readily understood by students.

Perhaps the best part of the book is an attempt which is made to detect pathological changes in the tympanum during life by a careful comparison of the physiology of the condition as observed in the case of a healthy and diseased ear. For example, in the symptom of better hearing following a sustained effect of listening, and the loss of the power of adjusting the ear to the speaker's voice, impaction of the stapes is considered by Dr. Allen to be indicated. The explanation given of the not very uncommon symptom of hearing better in a

[1] *Lectures on Aural Catarrh, or the Common Forms of Deafness and their Cure.* (Mostly delivered at St. Mary's Hospital.) By PETER ALLEN, M.D., Fellow of Royal College of Surgeons, Edinburgh, M.R.C.S., Aural Surgeon to, and Lecturer on Aural Surgery at, St. Mary's Hospital, London.

noise is, that it depends on are laxed state of the membrana tympani, that the sound acts as a stimulus to the auditory nerve, thus exciting reflex action in the tensor tympani, which by contracting, and thus making tense the membrane, improves the hearing. This is an ingenious solution of the question, but before it can be accepted as being invariably true, it must be shown that this symptom is not present in cases where no relaxation can be detected.

A just tribute is paid to Dr. Adam Politzer for the introduction of his method of inflating the tympanum, and it is pointed out how a large number of cases for which before there was no effective plan of treatment except the Eustachian catheter, now recover; whereas, formerly, owing to the difficulty of using it in the case of young children, they were not relieved.

It cannot, however, be very flattering to Dr. Politzer to find that a valve placed at the upper part of the bag instead of as it usually is at the lower part, and the adaptation of a nose pad (which many may not think so convenient as the india-rubber tube which is placed in contact with the floor of the nares) should transform at once the bag used in this proceeding to the "improved Politzer bag." At p. 79 we read:

"I must caution you, however, against inferring too much from the sounds produced by this mode of inflating the drum. I see that statements have been put forth which are not warranted by further experience in the use of this method. The otoscope placed in the meatus during this process rarely furnishes such satisfactory evidence with regard to the entrance of air, or the condition of the tubes and middle ear, as is afforded during the use of the catheter; for the noise made by the contraction of the muscles in swallowing the water, and that of squeezing the bag, mask the slighter sounds within the tympanum."

But in relating a case at p. 214, we find "on using Politzer's bag air entered with a moist flapping and crackling sound, very audible through the otoscope." Further on in the same case, "I again used the Politzer's bag and the sounds indicating moisture were less audible than on the previous day;" and at p. 219, in Case 3, it appears that "air rushed into the tympanum with a gurgling noise, and into the left with a clapping sound." From this the author would seem to have seen reason to modify his views on this point as the lectures proceeded.

The mode of action of the plug of cotton wool in cases of perforation of the membrana tympani is conclusively shown to be due to the support which it gives to the ossicles. With reference to the treatment of cases of chronic catarrh of the middle ear, a quotation is made in which Dr. Politzer advises injections into the tympanum and the introduction of elastic bougies into the Eustachian tube.

After mentioning that this line of treatment is adopted by one English aural surgeon (with 'Holmes' System of Surgery' before us, it is not difficult to guess to whom he refers) the author characterises it as "mistaken treatment," and exemplifies his opinion by saying that cases have been brought to him where the hearing has been very disastrously affected in consequence of this treatment having been pursued. However, in the hands of Dr. Allen, in similar cases, improvement is generally affected with ease by injecting through the catheter weak alkaline solutions.

When speaking of the operation of incision of the membrana tympani for chronic catarrhal effects, such as mucus in the cavity of the tympanum, which Dr. Allen says "has been rather extensively (in some instances) I think very rashly adopted in London," he continues, "I have myself witnessed cases in which its evil results have been conspicuous, and permanent deafness with difficulty averted." Whatever may be the author's opinion on this or any other question in surgery, and, by the way, he does not say what is the amount of experience he has had of this plan of treatment either in his own practice or that of others,—his views will assuredly carry more weight with the profession if put forward in such a manner as will avoid the semblance of an attempt to depreciate the practice of other surgeons. It is obviously no part of our duty to discuss here the merits of the operation in question, but, in pleasing contrast to these lectures, we notice an unobtrusive note to the supplement of Dr. Politzer's work on the membrana tympani which mentions that "when the membrane was much shrunken, and presented a dark greenish-yellow colour, Dr. Politzer, after using the air-douche with only temporary success, performed paracentesis, and immediately afterwards forced air through the ear by this method, thus driving the mucous matter out into the external meatus."

It is not the matter so much as the manner of these lectures that exception is taken to, and we cannot but think it a great pity that, where a good deal of information is plainly and intelligibly offered upon a subject about which not too much is known, a bad impression should be given of the spirit in which these lectures, in some parts of them, are written.

Annals of Cholera.[1]—This book is worthy of respect, as it comes from a gentleman who has had abundant opportunity for observing the malady of which he treats, and who, moreover, has evidently employed himself in much laborious research in elucidating the history of his subject. Nevertheless, we cannot conceal from ourselves that the general impression left upon the mind after a perusal of the

[1] *Annals of Cholera from the Earliest Periods to the Year* 1817. By JOHN MACPHERSON, M.D., Inspector-General of Hospitals, H.M. Bengal Army. Pp. 235 London: 1872.

volume is rather unsatisfactory, that many of the propositions which appear to be laid down by Dr. Macpherson are not sufficiently proved, and that a great part of the historical research is expended in obtaining an inadequate result. If we understand Dr. Macpherson aright, he professes to correct some erroneous notions which he thinks have prevailed as to the origin and the treatment of what is called Asiatic cholera; the first error being that the disease is a new one, dating from the year 1817 and appearing first in India, and the second, that a particular mode of treatment, now recommended, is a novelty, whereas it has been known and adopted for 2000 years. Dr. Macpherson accordingly devotes the greater part of his pages to an investigation of ancient records by medical writers in order to prove that cholera has existed among the nations of the earth, and has been recognised by physicians, from the most remote antiquity. But as to the second point, we confess that after carefully reading the book, we are still in doubt as to the nature of the supposed new method of treatment which Dr. Macpherson regards as an old one; for the author does not distinctly indicate what mode of treatment he refers to as being a pseudo-novelty, nor, on the other hand, does he show from ancient authorities that any particular or uniform mode of treatment was ever recommended or adopted by medical practitioners. As to his own method of treatment, or his own views of what the therapeutics of cholera ought to be, Dr. Macpherson is entirely silent.

Although it is impossible in the brief space at our disposal to give even an analysis of the laborious studies into which Dr. Macpherson has been led in his attempt to show that Asiatic cholera, as we now know it, was described and treated by ancient medical writers, yet we may adduce, as an instance of the inadequate reasoning which prevails in the book, the description of cholera contained in the works of Hippocrates. The great father of physic, as quoted by Dr. Macpherson, describes several cases of what he calls cholera, which undoubtedly present some of the features of the disease as we now know it, but we find that one case was soon cured by the use of hellebore and the juice of lentils; and that, moreover, Hippocrates describes a dry cholera (χολερη ξηρη) characterised by borborygmi and constipation. We must therefore agree with Dr. Macpherson in his naïve admission that the last is not cholera at all, and we may also express a doubt whether any of the cases cited by Hippocrates, and quoted by Dr. Macpherson, really represent the formidable epidemic malady which now occasionally decimates the nations of the earth.

On the other hand, while Dr. Macpherson labours to prove that cholera, or something like it, existed from all antiquity, and that it was not a new disease in 1817, he yet shows very clearly in his sixth and seventh chapters, that in the year in question the disease

began to excite general attentiou, and that several preliminary outbreaks in the East, at the close of the eighteenth century and the commencement of the nineteenth, had paved the way and prepared the public mind for the fearful epidemic visitation from the occurrence of which many persons date (as Dr. Macpherson thinks, erroneously,) the origin of cholera and its subsequent progress to other parts of the world. When the disease reached Jessore and Calcutta, Dr. Macpherson informs us, the medical authorities in the East entertained the same idea that he himself now advances, and regarded it as the usual epidemic of the season in an aggravated form, but "they soon acknowledged that the disease was a wide-spreading pestilence, of far greater severity than any annual epidemic that they had been accustomed to" (page 161). Dr. Macpherson goes on to show that the outbreak of cholera in 1817 in India was one of great magnitude, and prevailed over a great extent of country, and he also endeavours to trace the special causes which, in his opinion, intensified the visitation of the malady. It is therefore evident, from Dr. Macpherson's own statement, that a disease did break out in India, especially in Calcutta, in the year 1817, which caused extensive and unparalleled mortality, rapidly spread over a wide extent of country, returned in 1818 with still greater violence, and then, as we are all aware, travelled in subsequent years to Europe and the western hemisphere. We must accordingly express our conviction that Dr. Macpherson has expended a great amount of intellectual labour in endeavouring to establish a conclusion which is not one of primary importance; for even if we admit (and we confess that we cannot readily admit) that epidemics of cholera equal in magnitude to that of India in 1817, and followed by such extensive ravages in all parts of the world, have really occurred and have been described at former periods of the history of the world, yet this truth (if it be a truth) does not enlighten us either as to the nature, the causes, or the treatment of the malady. Nevertheless the investigation even of an abstract question in medicine has a great value of its own, and we should willingly agree with Dr. Macpherson in the conclusions he has drawn upon this point if we could conscientiously do so; but we think that, notwithstanding the great research which his pages display, he has not satisfactorily proved his case. It is not sufficient to show, for example, that some epidemic diseases have broken out in former times and have caused extensive mortality; but it is also necessary to show that these visitations were due to the disease we now know as Asiatic or spasmodic cholera, and this demonstration we think Dr. Macpherson has failed to give us.

The important question of the etiology of cholera, which has occupied the attention of the most skilful pathologists of our day, and has been solved in various ways with more or less probability, is dismissed by Dr. Macpherson in a very few sentences, and we

confess ourselves in doubt as to what his impressions really are on the subject. The quote his own words, however, he thinks that "we may conjecture it, at its commencement, to be some morbid process originating within the system, or excited in it by bodies organic or inorganic, solid or gaseous, however generated, coming from without" (note on page 167). But this view, we believe, is applicable to all epidemic diseases, according to Dr. Macpherson, who then proceeds in the same note to give a *catalogue raisonnée* of the views entertained by various authors as to the causes of cholera, such as contagion, the influence of air, water, soil, seasons, and weather; and after pronouncing all these causes to be more or less unsatisfactory, Dr. Macpherson abruptly quits the subject and leaves us in entire ignorance as to his own views on the matter.

We have observed that one object of Dr. Macpherson's book, as explained by himself, is to show that a mode of treatment, now recommended and considered to be new is really, like the disease itself, very old; but we have no chapter on treatment, and indeed, this part of the subject (at least, so far as modern therapeutics are concerned) is scarcely alluded to at all. The following remark, however, is valuable:—"Because an epidemic in the natural course of things becomes milder towards its termination, and at last ceases, the lessened mortality and final disappearance of the disease are ascribed to our improved practice, or to our better arrangements" (p. 199).

In an appendix, Dr. Macpherson introduces a paper, read at the Medical Society of London, on the "Analogies of Cholera Nostras and Cholera Indica," and if we clearly apprehend his meaning in this paper, it is intended to show that cholera *nostras*, or the disease known as Cholera Anglica (*nostras*, of our own country, *i. e.* England) is the same as the Cholera Indica, or the disease which prevails in India. He proves, with considerable success, that the symptoms of the two diseases are in many respects very similar; but whether he has satisfactorily shown that the slight epidemic bowel disorder which prevails almost every summer in our own island is the same identical disease as the fearful scourge which has prevailed (although happily at rather long intervals) in nearly all countries, in all seasons, and in all climates, we may be permitted still to doubt.

On Affections of the Heart and in its Neighbourhood.[1]—Dr. Dobell, in presenting this work to his readers, disclaims any attempt at abstruseness, which fault, indeed, he states that he has particularly avoided. His pages therefore are almost entirely of a practical character, and are occupied by a record of cases which have occurred

[1] *On Affections of the Heart and in its Neighbourhood: Cases, Aphorisms, and Commentaries.* (Illustrated.) By HORACE DOBELL, M.D., Senior Physician to the Royal Hospital for Diseases of the Chest. Pp. 128. London: 1872.

to him amongst his private and hospital patients, and are accompanied by aphorisms and remarks. The details of the cases are given with considerable minuteness, and display great industry in the collection of materials, but it may be perhaps a question whether some of them are of sufficient importance to deserve such lengthy descriptions as they receive at the hands of Dr. Dobell. One great deficiency, but for which the author is perhaps not responsible, is that they are mostly unaccompanied by the post-mortem appearances in the fatal instances, and thus their value is seriously diminished.

Although there is no systematic division of the book into chapters, Dr. Dobell distributes his matter into "Preliminary Remarks," in which he describes several points connected with the anatomy and physiology of the heart; the "Objects and Uses of the Heart-Bed," of which a plate is given; a section on "Clubbing of the Finger Ends," which occurs in heart disease, as well as in consumption, empyema, and chronic bronchitis, and of which condition photographic plates are also given; "Pain at the Heart and its Neighbourhood," comprising cases, aphorisms, and commentaries concerning the varieties in the character, seat, direction of extension, and duration of the pain and its collateral symptoms, a section which seems to us to be protracted to a somewhat unnecessary length; the "Interdependence of Affections of the Heart, Brain, Lungs, and Stomach;" and a very short postcript, in which Dr. Quain's lectures on the "Muscular Walls of the Heart" are noticed, and some of that physician's views are questioned or supplemented.

In reference to Dr. Quain's announcement that diseases of the heart are much more common in the present day than in former times, Dr. Dobell thinks that a considerable allowance is to be made for the frequency with which deaths used to be registered under the heads of dropsy, apoplexy, and paralysis, but are now attributed to heart disease; and he also thinks that among the causes of disease of the muscular walls of the heart, Dr. Quain has omitted to mention one which, according to his (Dr. D.'s) experience, occupies a leading position, namely, sexual excess.

Diseases of Hair.[1]—Dr. Godfrey tells us in his preface that he has devoted many years of study to the subject of the hair, in consequence of the demand made for more information on the matter on the part of the public. Although the book has for its title "Diseases of the Hair," Dr. Godfrey by no means confines his researches to pathology or therapeutics, but he plunges at once into an amusing though somewhat erratic essay upon the hair in all its aspects, and he tells us that he is indebted to the whole book-world for his information.

[1] *Diseases of Hair: a Popular Treatise upon the Affections of the Hair System, with Advice upon the Preservation and Management of Hair.* By BENJAMIN GODFREY, M.D., F.R.A.S. Pp. 183. London: 1872.

Literature, he says, is public property, like the air we breathe, so he has taken it and used it, and made it produce things new and old for the edification of his readers. "I have," he says, "culled flowers and fruit from the Bible, the Zendavesta, and the Talmud. I have gathered sheaves from the Greeks, the Romans and the Egyptians, and have gleaned stray ears of corn from every harvest-field where my feet have trod. I am indebted to all science for my facts, to all history for my information." This announcement describes pretty accurately the nature of the book, in which we find a profuse display of information of all sorts, new and old, sacred and profane, mythical and authentic, on the subject of the hair. There is, indeed, such a reckless display of statements, of reflections, of quotations, put together with very little attempt at order, and sometimes there is so much jocularity, that we are often in doubt whether Dr. Godfrey has any serious purpose in view in presenting his researches to the public; but we are bound to admit that the scientific aspects of the subject are handled with considerable ability, and that the treatment of the diseases of the hair, and the advice given as to its management and preservation, are presented in such a manner as to render the book useful as well as amusing.

Diseases of the Bones.[1]—Until the present book appeared no systematic treatise on the diseases of bone had been published in the English language since the classical work of Stanley; and as Dr. Markoe truly says in his preface, "much has been learned in bone-pathology" since then. We can but regret that the author has "not had the leisure" to embody more of this newly acquired knowledge in the present volume.

Dr. Markoe's treatise is chiefly clinical in its character, and on such disputed points of bone-pathology as the part taken by the bone-corpuscles in inflammatory changes, or the distinctions between true tubercle of bone and chronic osteitis with caseous degeneration, or between sarcomata and soft cancers of bone, we find nothing new or original, and a great deal that is very old. On the other hand, there are some interesting clinical facts and suggestions derived from the author's long and careful study at the bedside of the diseases of which he is treating. Under the name chronic sinuous abscess, Dr. Markoe describes a condition to which he seems to have paid particular attention. It consists of inflammation of bone, beginning as an acute attack, and passing rapidly into suppuration, resulting in an abscess which finds its way early to the surface; but owing to the opening not being sufficiently free, and probably not direct, the cavity does not close, but on the contrary, fresh

[1] *A Treatise on Diseases of the Bones.* By THOMAS M. MARKOE, M.D., Professor of Surgery in the College of Physicians and Surgeons, New York, &c. New York: 1872. Pp. 413.

abscesses form in the cancellous tissue in consequence of the irritation of the pent-up discharges. "By the repetition of this process, the bone gradually becomes the seat of an inflammatory hypertrophy, and is left perforated in all directions by sinuses generally communicating with one another with a central chamber." The disease is unaccompanied by caries or necrosis, and, if left to itself, shows no disposition to recovery.

The treatment adopted is simply to lay open all the sinuses by means of the trephine, chisel, and bone forceps, when granulations spring healthily from the bottom, and a rapid cure results. In illustration of this disease he gives some very interesting cases. As rickets seems almost unknown in New York, Dr. Markoe has relied mainly on Sir William Jenner's admirable lectures for an account of the disease.

In the chapter on caries the description of the microscopic appearances is borrowed from Mr. Barwell's work on joints, and contains a detailed account of changes commencing in the lacunæ. This account also is illustrated by a figure borrowed from Billroth, which is specially designed to demonstrate that author's view, that the bone-corpuscles remain totally unaltered until swallowed up in the cell-growth advancing from the Haversian canals. It is well to give both sides of a question; but it is only fair to let an author explain his own drawings.

Necrosis seems to be the subject to which Dr. Markoe has devoted the greatest attention, as, while tubercle of bone and caries only occupy thirty-four pages, necrosis extends over nearly one hundred. The author seems to have seen a remarkable number of cases of severe hæmorrhage caused by a wound of one of the larger vessels by a loose sequestrum. In the majority of these the bleeding was at first intermittent, and easily arrested in some cases, deceiving the surgeon and tempting him to the fatal mistake, against which the author warns his readers, of delaying operation under the supposition that only a small vessel was concerned. Dr. Markoe recommends immediate removal of the sequestrum, and that then an attempt be made to ligature the bleeding vessel in the wound, failing in which amputation is the only resource.

The internal tubular sequestra, so common after amputation of the thigh, the author believes to be due to division of the nutritious artery, so that the supply of blood is cut off from a portion of the medulla and wall of the medullary canal. He gives statistics regarding the position of the artery, but none as to the relative frequency of necrosis when the bone is sawn above or below the foramen, so that at present his theory requires proof.

Among the constitutional effects of bone diseases with prolonged suppuration, he makes no mention of albuminoid degeneration of the liver, kidney, &c.

Tumours of bone occupy nearly half the volume, and for their pathology the author is chiefly indebted to Sir James Paget; and for the chapter on tumours of the jaw to Mr. Christopher Heath. With the exception of myeloid sarcoma (which following Paget; he puts in a class by itself), he entirely ignores all modern observations with regard to the various sarcomata of bone, classing them all with soft cancers under the name "malignant tumours of bone." In fact, he even borrows from Billroth three drawings of sarcomata of bone, and uses them to illustrate soft cancer. He seems to have done this deliberately, and we cannot but think this confusion to be a retrograde step hardly justifiable in the present day.

The information in the whole book is to a very large extent compiled; the chief authors quoted being Stanley, Brodie, Paget, Billroth, Nélaton and Heath, and Barwell, and the greater part of the illustrations are copied from the works of Billroth, Erichsen, Paget, and Heath. The author in his preface says of the work, that "It does not claim to be a complete compendium of all that is known on the subjects of which it treats," and apologises on the ground of extensive practice and want of leisure. We can only accept his apology and thank him for so much as he has given us.

Galvano-Therapeutics. By W. B. Neftel, M.D. New York. 1871.—Books on electro-therapeutics require very careful sifting, for it depends much on the vigour of contemporary criticism whether this branch of therapeutics shall make a real and useful progress or whether it shall remain as it has been until our own generation, a very valuable and potent means rendered almost useless by the want of anything like accurate investigation and temperate handling.

Electro-therapeutics, being a very complicated and difficult study, are a snare to all men who have not accustomed themselves by early training to both accuracy and security of mental habits, and much rubbish is written and published upon the subject. It is impossible to class Dr. Neftel's little book under this latter head, for rubbish it certainly is not. The author, indeed, is one of the very few men we have met with who have really understood the force and bearings of Brenner's polar method and have endeavoured fruitfully to apply it. Brenner's great *forte* lies in the treatment of diseases of the ear, and this is at once a drawback and an advantage. The minute accuracy it requires is the advantage, but the drawback is that the field of his experience is confined to a department of which medical men generally know absolutely nothing, and the valuable results which he has won cannot, therefore, be appreciated beyond a very small circle of readers. Dr. Neftel has had the great advantage of studying Brenner's work at the fountain-head, namely in St. Petersburg itself; he has carried away a very intelligent and complete

[1] *Galvano-Therapeutics.* By W. B. Neftel, M.D. New York, 1871.

notion of it, and seems himself to continue it with skill and discrimination. But it is rather in its application to general therapeutics that our readers will wish to hear what Dr. Neftel has to say, and we are the more sorry as Dr. Neftel has given us in the present little treatise a mere fragment of a larger one which he has in preparation. It seems unfair and a waste of time to criticise the conclusions at which an author arrives when we have none or but few of the grounds before us on which those conclusions are founded. Still, in spite of this hesitation, we must say that when we leave ear medicine we do not find that style of handling which would give us confidence in the writer. For instance, the power of galvanizing the sympathetic in any direct and exclusive way during life has been gravely questioned by many observers, namely by Ziemssen in Germany, and by Dr. Clifford Allbutt in this journal; yet Dr. Neftel relies to a great extent upon this supposed possibility without adding anything important in the way of further proof. The one experiment he adduces seems to us so inadequate that it rather decreases than increases our faith in the writer. On this slender foundation, however, Dr. Neftel builds up a fabric of practice which unfortunately gives us, as is the case with so many treatises on electro-therapeutics, the same impression as the perusal of a pamphlet of Mr. Holloway, and we half unconsciously looked to find at the end of the several case-histories the patient's signature with a permission "to make any use of this you please." When will medical electricians learn that they must give us all their cases, good and bad together, if they intend to carry any conviction to the mind of the instructed physician?

Medical Thermometry.—Wunderlich's classical work has now been done twice into English—once by Dr. Séguin in America and once by our own Sydenham Society. In appearance, at least, the latter is by far the preferable volume, for the book now under review does not reflect much credit upon the printer, whatever we may grant to the editor. Concerning the first and chief part of the volume, which consists of the translation of Wunderlich's work, somewhat abridged by the translator, we need say little, the merits of that classical treatise being sufficiently known. The translation seems to be satisfactory enough. In a second part of the volume, however, Dr. Séguin appends some original matter of his own, which claims an especial notice at our hands. This we welcome as the evidence of useful activity in a new field, for there is so much to be done in medical thermometry that we have need of many workers. Dr. Séguin also, as a member of the active and intelligent brotherhood of American physicians, must be regarded by us with respect and

[1] *Medical Thermometry.* By C. A. Wunderlich. Translated, with additions, by Dr. Séguin. New York, 1871.

courtesy. At the same time we cannot see that his original contributions are likely to do much for the branch of science now under consideration. A great part of his appendix is occupied with the complaint that the thermometers used in medicine are not *medical* thermometers, but the tools of the physicist. By a medical thermometer Dr. Séguin would understand an instrument, the zero of which is placed at the normal bloodheat, all degrees above that being units of fever, all below units of algidity. Such an instrument, marked some dozen or so of centigrade degrees above and below a zero point corresponding to 98·6° of the Fahrenheit scale, he has constructed and calls it truly medical. Surely this is not only fanciful, but a mischievous whim. The great interest many of our best observers find in the study of medical thermometry is, that we are thereby enabled to bring many of the phenomena of life into direct comparison with physical phenomena, and to prove the common nature of the two. To set up an artificial barrier between them, to divide the physiological from the physical facts, even in appearance, seems to us a very retrograde step. Dr. Séguin, moreover, in imitation of the old-fashioned barometers marked "rain," "change," "set fair," &c., gives us his medical thermometer with storied margins thus:—5° to 6° (*c*), death; 3·5° high fever; 2° collapse, and so on, which, in our humble judgment, is very far from science. Another chapter of Dr. Séguin's appendix consists in a series of remarks upon surface thermometers; he admits that all observers hitherto have been disappointed in their endeavours to make such instruments of a trustworthy kind, but believes he has himself overcome the difficulties of the problem. When we come to learn in what way he has obtained so marked a success, we find it is by the use of the well-known button bulb, which, probably, every inventor in this department has tried and discarded. We have not found much pleasure or profit, then, we must confess, in the original chapters of this little book; but we would conclude with a word more of thanks to Dr. Séguin for his labours in endeavouring to popularise the results of the investigations of others.

The Fallacies of Teetotalism.[1]—It is very difficult to review this book, because the reviewer may look at it from two rather different points of view. He may look at its value either as a permanent contribution to the study of the question with which it deals, or as a temporary help in fighting a battle in which hard knocks are given and taken. As a permanent contribution to the study of teetotalism as a medical or social question Mr. Ward's volume can take no rank. Both facts and arguments are of that rough and ready kind which do almost better than the best for purposes of agitation and protest,

[1] *The Fallacies of Teetotalism.* By ROBERT WARD. London, 1872.

but which could not stand any degree of careful sifting or discussion.

Regarded as it is, as a vigorous piece of polemics, it is very effective, and will serve a good purpose. We as advocates of temperance, and therefore opponents of teetotalism, have a kindness for the book as a bold and honest attempt to stem a vast rush of ignorance and fanaticism, and we think it will do a great deal of good as a popular rally. Too many of us are ashamed to resist the narrow and mischievous policy of abstainers, and to argue against their essentially ascetic, and, therefore, dark and retrograde theories, lest we should be accused of sympathy with drunkards and indifference to the miseries which will come of intemperance. Let it be distinctly understood, as Mr. Ward boldly proclaims, that a man of temperance is as out-and-out an enemy of the drunkard as he is of the teetotal man, and thus the cause of the golden mean will prosper.

Reports of the Rudolph Hospital, Vienna.[1]—We have received these reports with pleasure, as they seem to us eminently useful and more to the point than the ambitious volumes which issue from the English hospitals. They are small books, and are occupied entirely by actual accounts of hospital work, pointed here and there by brief annotations by the physicians and surgeons of the institutions. In short, they more resemble a published collection of the hospital reports of our journals than the collection of essays initiated by the staff at Guy's Hospital. We have no reason to complain that such handsome volumes issue from our press, and that the heads of the profession deliver themselves annually of such excellent discourses; still the great multiplication of archives is a nuisance to the student, who would lose less time could he count on finding these essays in the recognised journals and reviews, whose pages are always ready to receive them. The hospital reports, meanwhile, would contain their own special contributions, which the student would seek in their right place.

The volumes before us contain the usual tables, professional and economical; a detailed list of the cases which have been under treatment, with brief remarks upon each class of them, and a very readable and valuable collection of special case-histories, with notes on individual cases by the medical officers of the charity. These annotations seldom occupy a page, sometimes only three lines, but they are pithy, and often contain matter of great interest. We take the following at random from annotations on "Neubildungen," which scarcely fill two pages, and which come partly from the hand of Dr. Drasche, and partly from Dr. Mader. Dr. Drasche says briefly as to case-history (No. 16) of cancer of the mediastinum:—"I believe that we were able to recognise its probable cause as traumatic.

[1] *Bericht d. k. k. Kranken-Anstalt Rudolph-Stiftung*, in Wien, von Jahren, 1869 and 1870.

According to my experience this possible cause has been brought wholly into discredit on mere *à priori* and inadequate grounds."

Lectures on the Clinical Uses of Electricity.[1]—This excellent little volume is far superior to that of Neftel, and, indeed, there is no small book extant which can at all approach it in value. It is not necessary to do more than cut the pages in order to see that we have something better to deal with than a mere search after novelties, or a record of wonderful cures.

Dr. Reynolds knows that electricity is no panacea; he knows that cases treated by electricity often disappoint us sadly; he knows that as yet we are very much in the dark concerning its mode of relief and the methods of its management, even in the simplest cases, and, accordingly, we find his pages marked by the true caution and the equable argument which becomes the thoughtful and instructed physician. Dr. Reynolds, moreover, gives due place to some of the uses of electricity, which are none the lower for their reference to something other than immediate cure.

Thoughtless or ignorant persons rush into cures of the first water after airing a little second-hand and defective learning concerning the physics of electricity in the introductory chapters. Dr. Reynolds has the sense of fitness, which teaches him that elaborate descriptions of instruments, and so forth, do not serve the practical needs of the student; but he devotes the time spared from this to a very succinct and accurate description of the uses of electricity in diagnosis. Indirectly, of course, this use of the means has the most important bearing upon treatment itself, and lies on the threshold of all well conceived therapeutics. No better illustration of this truth could, for instance, be had than the two well reported cases of impaired writing power given in the August number of the 'Practitioner,' by Dr. Buzzard.

Another good point one quickly seizes in Dr. Reynolds' little work is, that he sees through all shaky reasoning. We find no system of treatment built upon such sandy foundations as galvanization of the splanchnic nerves, and it is quite refreshing to read the treatise through without once coming across "galvanization of the sympathetic," save in a rather contemptuous allusion on the very last page, where the author says:—"The evidence upon these matters is of such a sort that I think it better merely to allude to the application than to describe its method in detail."

This little book is, therefore, just the thing, just the simple, accurate, safe, readable thing for the enlightenment of the great mass of the profession; intelligent medical men will find in it how important electro-therapeutics really are, and how necessary it is

[1] *Lectures on the Clinical Uses of Electricity.* By J. Russell Reynolds, M.D. London, 1871.

that they should have some knowledge of its methods, and they will be enabled to practice these methods, or to enter into their practice by others, with the clouds of sciolism and quackery dispelled which seem for ever to haunt the subject.

We have only to regret that so able an author has allowed his name to be published in the advertisements of Mr. Pulvermacher, which are full of pretence and inaccuracy, and which appeal not to the educated professional man, but to the ignorant general reader.

Dr. Tanner's Memoranda on Poisons.[1]—The editor of this new edition, whose name does not appear, informs us in his preface that, as experience has shown this little book to be more appreciated by students than by practitioners, he has introduced such changes in it as are calculated to make it still more useful to the former class of readers. Among those changes are enumerated greater attention to the chemical characters of poisons, to tests and to processes for separating poisons from organic mixtures. He has also adopted a classification based on that used by Dr. Guy in his work on 'Forensic Medicine,' but slightly modified.

This last-named novelty is, however, in our opinion, not likely to be regarded by students with particular favour, as its divisions and subdivisions represent so much more perplexing phraseology, and it adds yet another system of classification to be acquired, which, like those that have preceded it, are more ingenious than useful in practice.

The approval signified by the call for a third edition indicates that these memoranda on poisons furnish a sort of information that is needed. At the same time, they demand no critical examination, being nothing more than excerpta from well established works on forensic medicine, toxicology, and chemistry. The only important point to determine is whether the selection made is the best for the purposes of the work. On this head, we may state it is sufficiently good, although the instruction relative to tests and antidotes is in many places superficial. Various omissions and shortcomings could be pointed out. We will just note the omission of cyanide of potassium, a poison now much in use by photographers, and one which has proved fatal in several cases. The account of the anæsthetics, moreover, is very meagre, and the antagonistic action of certain poisons receives no notice. With all its defects, however, this little book contains a large amount of useful information relative to poisons, and recommends itself to students by its convenient size and its presumed sufficiency for securing their passage through their examinations in toxicology.

[1] *Dr. Tanner's Memoranda on Poisons.* By the late THOMAS H. TANNER, M.D, &c. Third and completely revised Edition. 1872.

Grainger Stewart on Bright's Disease.[1]—The character and merits of this volume were pretty fully set forward in a review of its first edition, and it only remains now to state that the present issue is considerably enlarged, and in respect to many particulars improved. The enlargement and improvement are specially noticeable in the clinical records of cases and the comments thereon, and hence the treatise will bear out even more fully than its first issue did, its practical character. Moreover, its value practically is enhanced by the greater and more lengthened opportunities afforded the author for the investigation of cases since he produced the first edition.

On the other hand, little has been done to make it a fully representative treatise in the matter of renal pathology. There are, indeed, some new plates added of much value in elucidating the morbid appearances in the several lesions comprised under the head of Bright's disease, but we are disappointed at the meagreness of the additions made to the pathological details as contained in the former edition, and at the omission of reference to the work done by various recent pathologists and microscopists. For instance, the cardiac hypertrophy is attributed, as in the former edition, to the production of the one factor, the retention of effete matter in the blood from defective elimination of the kidneys; and no notice is taken of Dr. George Johnson's researches relative to the grave alterations of the small arteries generally in calibre and wall-thickness.

The classification adopted remains the same, as does also the subdivision into chapters, except that two new chapters are added, one on the differential diagnosis of the different forms, and a second on the complications of the several varieties of Bright's disease. The latter chapter is statistical in character, and has been reprinted from the pages of this Review for 1867.

Although compelled to indicate some shortcomings in this new issue, we nevertheless owe it to the author to state that as a practical clinical treatise on Bright's disease it is not surpassed.

Siebold on the Parthenogenesis of Arthropoda.[2]—In this valuable work V. Siebold gives the results of several years' patient examination of various articulate animals in reference to the production of a progeny by the females without sexual intercourse. The longest paper is devoted to the Polistes, a species of wasp not indigenous in England, but common in middle Europe. Other papers treat of parthenogenesis in *Vespa holsatica;* in *Nematus ventricosus;* in *Pysche helix;* in *Solenobia triquetrella* and

[1] *A Practical Treatise on Bright's Disease of the Kidneys.* By. T. Grainger Stewart, M.D., &c. Second edition. 1871.

[2] *Beiträge zur Parthenogenesis der Arthropoden.* Von C. Th. E. von Siebold, Leipzig. Engelmann, 1871. pp. 238.

lichenella; in *Apus cancriformis* and *productus*; in *Artemia salina* and in *Limnadia Hermanni*.

He gives many interesting particulars respecting the life history of the Polistes. The fertilised queen Polistes overlives the Winter, and with the first warm days of spring wakes up and sets about the construction of her nest. Having built about fifteen or twenty cells she deposits her eggs, which for the most part develop into perfect though rather smaller females than herself, and one or two males. She is then fully occupied in procuring food for the larvæ. After the lapse of three weeks or more according to the temperature, the females having passed through the chrysalis stage escape, and at once aid the queen in extending the nest. They also deposit eggs in the cells, and V. Siebold has ascertained that all these parthenogenetically laid eggs develop into *males*.

His experiments and observations have further led him to the following conclusions in respect to the cause of the production of the two sexes: (1). The ova carry or bring with them from the ovary the capacity of differentiating themselves into male animals, and (2) of developing into such males quite independently of the sexual influence of a male parent; but (3) these same ova if subjected to the influence are no longer developed into males, but into females.

The development of parthenogenetic ova into males occurs also in *Vespa nematus* and in Apis, whilst in Cochlophora and Solenobia, in Apus, Artemia, and Limnadia, the parthenogenetic ova develop exclusively into *females*. He accepts the term "Arrenotokia" invented by Leuckart to indicate the process occurring in animals that like the Apidæ, Vespidæ, and Tenthredinidæ, amongst the Hymenoptera have male parthenogenetic offspring, and suggests the employment of the term "Thelytokia" to represent the production of female parthenogenetic development as occurs in the Lepidoptera and the Crustacea, in Psychidæ, Talæporidæ, and Phyllopoda.

Milne-Edwards' Lectures on Comparative Anatomy and Physiology.[1]—We are glad to see after so long an intermission another part of this most excellent work, perhaps the only one of its kind in which French lucidity of expression is combined with German exactness and astonishing wealth of bibliographical reference.

The present part is occupied with (1) an account of the tegumentary system of animals, generally including description of the skin of vertebrata with its appendices of hair, pigment, nails, osseous scutes, glands, and feathers, and (2) an account of the integuments of invertebrate animals, embracing descriptions of the investing membrane of the Infusory animalcules, the sclerenchyma of the Zoophytes,

[1] *Leçons sur la Physiologie et l'Anatomie comparée de l'homme et des Animaux*. Par H. MILNE-EDWARDS. Tom. x. Première partie;—Système Tegumentaire.

the spiculæ of Sponges and Radiolaria, the skulls of Foraminifera, Echinodermata and Mollusca, and the hard parts of the Vermes, Myriapods, Insecta, and Crustacea. The last chapter (eighty-ninth) begins the consideration of the interior skeleton or hard framework of Vertebrata.

We append a single quotation which will give those of our readers who have not seen the work an idea of the mode in which each subject is treated.

"In the class of Arachnida the tegumentary skeleton is much less consolidated (than in the Myriapoda). It may acquire a consistence about equal to that of horn, though not unfrequently the skin preserves a certain softness or suppleness over a great part of the body; but it is especially important to note the grouping of the zonites that characterises this great division of Articulata. Here the head is always blended more or less perfectly with the trunk, and this instead of exhibiting the same mode of conformation throughout its length is only provided with limbs in its anterior portion, called the *thorax*, which consequently becomes perfectly distinct from the posterior portion of the body named the *abdomen*. In the Arachnida then the body is composed of a cephalo-thorax and an abdomen. Nevertheless the polyzonary composition of both these divisions is easily demonstrable. Thus amongst the Araneidæ where this mode of constitution of the tegumentary system is obscure in the adult animal, the division of the body into a series of segments is one of the first phenomena by means of which the Embryogenic process manifests itself in the interior of the egg. In the first outline or sketch of the young animal in process of formation two polar caps appear corresponding to the parts destined to become the cephalic region and the anal region, then a series of intermediate bands directed transversely and representing the sternal arc as well as the tegumentary rings. At first there are six which belong to the cephalo-thoracic region of the body, then two others, which become constituents of the abdomen and soon their number rises to ten. But these zonal bands have only a transitory existence, soon they blend together, and then the annular division is only recognisable by the appendices that arise in pairs from the cephalo-thoracic region of the body, and which appear in the first instance in the form of six pairs of small tubercles symmetrically disposed at equal distance from one another between the two caps above mentioned; a pair of tubercles appear also in the frontal region, and these diverse appendices, at first similar, become by degrees more and more differentiated till they assume the proper characters of the *chélicères* or *antennes-pinces*, the buccal organs previously described, and the four pairs of legs which are attached to the thoracic region of the perfect animal. In this last the dorsal portion of the tegumentary forms only a single solid piece in the cephalo-thoracic region which supports the eyes above, gives insertion to the chélicères anteriorly, and articulates with the base of the feet laterally. Inferiorly there is a small sternal plastrum situated between the basal segments of the ambulatory legs,

and a median plate of the same kind placed between the bases of the feet-jaws. Lastly apodemic prolongations occur in the interior of the thorax. As regards the abdomen its tegument remains soft and only rarely presents feeble traces of segmentation; at its subterminal part, however, the articulated appendages are found which enter into the composition of the secretory apparatus of the silk, and which are comparable to the members of which the genital arinature of insects are formed."

He then compares the conformation of the skeleton in Phrynidæ, Scorpionidæ, and Thelyphonidæ. Amongst the authors that M. Milne-Edwards quotes in this section are Claparède, Herold, Blanchard, and Bourguignon. The author appears to have spared no pains to make the present as complete as the previous volumes, many of the references being to essays published during the last year or two, and some, as in the case of Dr. Carpenter's paper on the *Eozoon Canadense*, published in journals ('The Intellectual Observer,' for instance) which were scarcely likely to come under the notice of a foreigner. We trust the work will now proceed uninterruptedly to its conclusion.

Nicholson's Introduction to Biology.[1]—This is a well-conceived, and, as it is almost needless to say to those who know the character of Dr. Nicholson's previous works, a well-executed production, well fitted for its purpose as an introduction to the study of biology.

The author virtually disarms the criticism that might be exercised in the way of pointing out omissions on one or other topic adjudged to belong to the subject-matter of his book, by remarking in his preface that "many important subjects have, of course, been necessarily treated very superficially, or altogether omitted, as unsuitable for a merely elementary work." Nevertheless, he cannot expect his critics to forego entirely the exercise of their craft; and if we object that some further use might have been made of vegetable anatomy and physiology in elucidation of biology, and that some more notice might have been taken of the encysting process and of the resting stage among the lower organisms, we are confident he will accept the criticism with the best grace. The taste thus secured to ourselves in the exercise of our natural function begets, unhappily, a further desire to find fault, to gratify which we fall foul of some of the illustrations, which are coarse, hard, and indifferent copies of engravings previously produced, and also not so accurate in the delineation of details as could be wished.

It is also right to observe, what, indeed, is candidly stated in the preface, that much matter is here reproduced which has appeared in previous works of the author, and particularly in his 'Manual of Zoology.'

[1] *Introduction to the Study of Biology*. By H. ALLEYNE NICHOLSON, M.D., &c. London and Edinburgh, 1872. Pp. 163.

However, we have read the book with much satisfaction, and find in it just that sort and amount of information needed by students when first taking up the subject of biology. The writer has taken great pains to fairly exhibit the leading doctrines of the science without obtruding upon his readers too much of the controversial matters that now divide and somewhat embitter antagonists. His own opinions are essentially eclectic, and prove him to be not a disciple of the advanced pangenesists and evolutionists, nor a partisan of other extreme views. Such a quality especially befits a writer for students.

The Half-Yearly Abstracts.[1]—It is enough to announce to our readers the regular appearance of these well-known and much valued retrospects of the medical sciences. We are pleased to observe in Dr. Stone's abstract a fair sprinkling of extracts from foreign authorities, a feature that renders it of wider interest and a more fitting representative of what is passing in the medical world than the rival publication by Dr. Braithwaite, which is a reflex of British medicine almost alone. In general, however, the latter presents what will be considered in the eyes of very many practitioners somewhat more detailed abstracts, and a selection of papers distinguished by more attention to practical questions and to matters of treatment. In short, each publication exhibits, as it should do, an individuality of its own, and the length of time the two works have been in circulation proves that each has its appreciative clientèle.

Principal Varieties of Pulmonary Consumption.[2]—As the substance of this small treatise appeared in the form of a series of papers in our contemporary 'The Medical Times and Gazette,' and, consequently, many of our readers are conversant with the views of Dr. Powell on pulmonary consumption, there is no need for a lengthened notice. But it is at the same time with much satisfaction that we call attention to this volume. It presents a carefully drawn up history of the several varieties of pulmonary phthisis, now generally recognised, illustrated by cases and accompanied by remarks on treatment; and its teachings, both pathological and therapeutical, will be received with the deference due to them as coming from a physician who has made phthisis a special study and had special opportunities for studying it.

He urges more attention to the recognition of the different forms of the disease as a practical desideratum; for, as he remarks, the usual view of it arrived at by students during their hospital career,

[1] *The Retrospect of Medicine.* Edited by W. Braithwaite, M.D., and James Braithwaite, M.D. Vol. lxv, January to June, 1872. London. Pp. 415.
The Half-Yearly Abstract of the Medical Sciences. Edited by W. Dorrett Stone, M.D. Vol. lv, January to June, 1872. Pp. 372.

[2] *On the Principal Varieties of Pulmonary Consumption, with Practical Comments on Diagnosis, Prognosis, and Treatment.* By R. Douglas Powell, M.D. &c. London, 1872. Pp. 96.

that it is "a disease of long duration and of ultimately fatal issue, in which, the diagnosis once made, the prognosis is a matter of common inference," is insufficient for the exigencies of future practice and leads them to annoying errors, particularly in the matter of prognosis.

He enters upon his subject by a general review of the processes concerned in the production of phthisis, and gives an excellent sketch of catarrhal pneumonia and of the tissue results consequent thereon. He considers the evidence now collected is sufficient to uphold the doctrine "that tubercle is a morbid growth of the lymphatic-gland class, and that it may be a mere hyperplasia of the gland tissue normally minutely disseminated through the organs of the body," and consequently that it is not to be classed among the ordinary products of inflammation. He moreover entertains the solacing belief that tubercle is much more commonly a secondary disease; and that though hereditary tendency has some effect in its development, it has far less than supposed. To respiration considered as a muscular act he assigns a much higher importance pathologically than is usually done, making the muscular movements of the chest, when disease is going on in the lung tissue, accountable for pleuritic thickening.

The following chapters are taken up with the descriptive pathology of alveolar catarrh, catarrhal pneumonia, and the specially named forms of phthisis, viz. fibroid and hæmorrhagic, consumption with recurrent hæmoptysis and tubercular phthisis.

On the subject of fibroid phthisis Dr. Powell is inclined to accept the opinion of Dr. Wilson Fox that pulmonary fibrosis occurs as an occasional morbid change without tubercle, and as a form of chronic pneumonia. There is no question that such a pathological condition exists, particularly as a consequence of the mechanical irritation of dusts inhaled, and having a clinical histology of its own. This chapter on fibroid phthisis is chiefly occupied by the details of a case, with accompanying clinical remarks and a diagram. There is some confusion in the descriptive part of this case between the left and right side, which should be corrected in a future edition. A proper distinction is made between hæmorrhagic phthisis and phthisis with recurrent hæmorrhage, restricting the former term to cases in which the disease appears due to antecedent hæmorrhage.

Sufficient has been said, we believe, to justify the opinion expressed at the commencement of this notice.

Natural Philosophy for General Readers and Young Persons.[1]—Many are the attempts now a day to find out a royal road to learning, to smooth down the difficulties surrounding every science, and to help the student over every impediment in his course. Such

[1] *Natural Philosophy for General Readers and Young Persons.* Translated and edited from GANOT'S *Cours élémentaire de Physique.* By E. ATKINSON, Ph.D., &c. London, 1872. Pp. 522.

attempts are, on the whole, commendable, although we are not sure but that learning is better learnt when the student is less helped and is thrown more on his own resources. We believe that one so placed gains in mental discipline and in power to grasp every subject which he encounters.

However, this is not a question to be debated on the present occasion. We have before us a work which places before the student the great facts of natural philosophy in a manner most readily to be received and comprehended.

It is profusely illustrated; scarcely a fact, however simple, in which some ordinary process can give experimental evidence failing to be figured, whilst the mode of experimenting in every subject treated of is largely shown.

The work comprises an account of the general properties of matter, motion, and force, with the mechanical powers, hydrostatics, the physics of gases, acoustics, heat, light, magnetism, and electricity. "In range" (as Dr. Atkinson observes in the preface) "the book may, perhaps, be fairly taken to represent the amount of knowledge required for the matriculation examination of the London University."

Should its sufficiency in respect of this particular examination as a text-book be found deficient, it would be, we apprehend, in the subjects included under the head of "Mechanics," problems in which are sometimes given which require more mathematical treatment than this treatise places before its readers. On the other hand, in regard to this self-same examination, the chapter on acoustics, magnetism, and electricity, would be redundant.

However, although Dr. Atkinson has kept in view the wants of students preparing for the London University examination, his work addresses itself to a still wider class of readers—indeed, to every intelligent individual who desires to understand something of the phenomena of nature incessantly passing before his eyes, as well as something of the many applications of natural forces devised by man for his convenience and happiness.

As a book for beginners in the study of natural philosophy, we do not know a better.

Observations upon the Climate of Uckfield.[1]—The production of this treatise is a praiseworthy undertaking on the part of its author. For twenty-eight years he has assiduously observed and noted the meteorological conditions of the rural district in which his lot as a surgeon has been cast, and this work he has done heartily and with scientific care and accuracy. He has had the advantage of a properly constructed observatory, and has been able

[1] *Observations upon the Climate of Uckfield, constituting a Meteorological Report for the District from* 1843 *to* 1870, *Prognostics of Atmospheric Changes, and some Vital Statistics.* By C. Leeson Prince, M.R.C.S., &c. London, 1871. Pp. 239.

so to place his instruments as to secure their readings from error, and from the whole tenor of his book it is evident that the work he has done has been one of love, and very rightly he looks upon such work as of very high importance, and worthy of finding imitators among medical men at large by reason of its many bearings upon health and life.

He justly observes, "The atmosphere and its changes which immediately surround us, and influence to a great extent our daily pursuits and avocations, are really less understood than many other subjects of far less interest and importance."

There is so much information given in the several chapters on the barometer, the hygrometer, and other instruments, and on the nature and influence of atmospheric changes, that the treatise has to some extent the value of an introduction to meteorology. Nevertheless it is obviously not his intention to make it such, but rather to restrict his undertaking, and make it a record of observations made by himself.

After some introductory remarks on the earliest efforts in meteorological science, on the situation and surroundings of Uckfield, and on the arrangements made for conducting his observations, Mr. Price gives a very instructive chapter on the barometer and atmospheric pressure, accompanied by tables for calculating altitude by means of barometers. He then commences his record of the general meteorological character of the several months of the year, and adds thereto various tables relating to local temperature. Moisture, rain-fall, and wind constitute the subjects of the next chapter, and are illustrated by tables of results of observations.

The next chapter, the longest in the book, is occupied with "Monthly remarks respecting atmospheric phenomena from the year 1843 to 1870, both inclusive." This may be regarded as the transcript of his diary. Although this section of the work is chiefly occupied with the local meteorology of Uckfield, it contains frequent references to that of other regions, and now and then notes on incidental circumstances connected with the seasons, such as the first appearance of certain birds. This portion of the work will be of most interest to those who have kept similar records, by way of comparing observations.

Chapter V will have a wider interest. It is on prognostics of atmospheric changes, and introduces the teachings of the very little known ancient Greek poet Aratus, who wrote about 270 years before Christ. Every reader will peruse the translation given with much pleasure; and with respect to the lists of prognostics of the weather, not a few readers will be prepared to add some of their own. The concluding chapter is occupied with health statistics of the district, which, if on the whole is indicative of a generally healthy state of the population, show an excessive death-rate with regard to certain maladies, among which pulmonary consumption may be cited.

Original Communications.

1.—On Retrovaccination.—By P. M. Braidwood, M.D.

Another form of preventive inoculation which has been strongly advocated by some, and which is at the present day carried on under the direction of the Austrian, Bavarian, and Würtemberg Governments, is termed *Retrovaccination*.

About a quarter of a century after the introduction of vaccination and its diffusion over the world, a very general conviction was felt by vaccinators in different countries, especially on the continent of Europe, that the vaccine virus had become less protective than it was originally—that it had degenerated by continuous successive transmission through the human subject. This led to numerous experiments being made, with the object of renewing the vaccine lymph by causing it to pass again through the bovine economy. The inoculation of a cow with humanised vaccine, and the vaccination of children with lymph from such a cow, is *retrovaccination*. Innumerable experiments of this kind have been made. Mr. Ceely performed several discriminative experiments. He used, in the first place, lymph which had been for a long time continuously transmitted through the human subject; 2ndly, some humanised lymph which had been derived from the cow about two years before; 3rdly, some humanised lymph of the nineteenth remove, which had been originally produced in the cow by the process of "variolation;" and, lastly, some retrovaccine lymph, or humanised lymph, which had been passed through a cow, then transferred to the human subject, and again taken from the human subject at the fifth remove and engrafted on the cow. These various methods led to the same result, viz. success—perfect and productive vesicles—in every case where the lymph itself was active, and irrespective of the source from which it was derived, and at however many removes from its spontaneous origin in the cow.

Against retrovaccination, as against animal vaccination, the principal objection raised has been its uncertainty. Very conflicting statements have been made as to the facility with which cows may be successfully inoculated with humanised vaccine lymph. The attempts to vaccinate cows with humanised lymph made in 1818 and in 1826 under the direction of the Würtemberg Government failed. According to Heim, Hering, and Hausmann, this experi-

ment is successful only once in ten or twelve trials. Fiard observed only six or seven very doubtful successes in seventy experiments. Viborg and Ritter failed in all their experiments. "On the other hand, Caqué at Rheims, Husson and Tenier at Paris and Versailles, Sacco at Milan, and Hellwag at Eutin in the early days of vaccination, and more lately Numann at Utrecht, Billing at Stockholm (in September, 1832), Lentin at Weimar (in 1835), Prinz at Dresden (in 1838), and others, have experienced no difficulty. Carganico of Darkehmen succeeded every spring for several successive years; Magliari of Naples professes to succeed "journellement;"[1] while that careful experimenter and distinguished authority M. Bousquet has experienced perfect success in his experiments on retrovaccination. Drs. Zybel, Nicolai, With (professor at the veterinary school in Copenhagen in 1835), Steinbrenner, Boutet, Axisa of Malta, and Maunoury of Chartres, have also produced cowpox by vaccinating cows with humanised vaccine. Some of these experimenters were content with watching the effect of vaccinating cows with humanised lymph; while a few of them, especially Drs. Carganico, Prinz, Hering, and Bousquet, pursued the process further and tested the vaccine thus generated by applying it to the human subject.

Success in retrovaccination, as Ceely has observed, depends greatly on the age of the lymph employed; and also, as Bousquet and Ceely remark, on the choice of the animals to be experimented on. M. Chauveau and his colleagues agree with Ceely that the length of time that the vaccine has been humanised makes no difference as to its taking effect when transferred to cows. These French observers on comparing the effects on cows of long humanised lymph with the effects of cowpox lymph which had not been humanised (as they supposed it to be, but which was in reality retrovaccine lymph), and which had undergone a series of transmissions through the bodies of animals, found no difference between these two varieties of lymph, either as to the facility of their acting or as to the effects produced. Many cows are affected with difficulty, or are altogether insusceptible to the ordinary modes of vaccinating. Mr. Ceely, for example, made incisions into the cellular tissue and inserted crusts of natural cowpox, and the wounds healed without any result. He observed, however, "the greater difficulty of vaccinating the cow with humanised than with primary lymph, and that, when successful, a much milder disease is the result." He concludes, therefore, that vaccine lymph in passing from the cow to man undergoes a change which renders it less susceptible by, or less active in, the bovine species which produced it. "But this reluctance or absolute refusal," says Ceely,

[1] E. C. Seaton: 'A Handbook of Vaccination,' 1868, p. 21. E. Hering: 'Ueber Kuhpocken am Kühen,' Stuttgart, 1839, pp. 17 et seq. C. G. Prinz: 'Praktische Abhandlung über die Wiedererzeugung der Schutzpockenlymphe durch Uebertragung derselben auf Rinder,' &c., Dresden, 1839.

"this difficult imbibition or total insusceptibility, does not exist to the same extent as observed in the converse of this experiment, namely, in passing primary lymph from the cow to man by the same means. Here we have considerable difficulty; but when we succeed a severer disease is induced."[1]

In May, 1869, I vaccinated six cows, which had calved but were not then giving milk, with humanised lymph which had been preserved for a short time in capillary tubes. Each cow was vaccinated by scratches in two spots, and a little blood was drawn to make sure that the skin was sufficiently penetrated. To each spot a tube of lymph was applied and the animals were watched carefully from ten to fourteen days. The operation failed in all the cases. The owner of the cows, however (a very intelligent farmer), informed me some months afterwards that one of these cows showed *one* distinct vaccine vesicle, which ran its normal course, and that its crust fell off after an interval of three to four weeks. The animal's health was not at all affected by the disease: he had watched with interest the course of the vaccinia.

In June, 1870, I vaccinated five cows which were being milked. On one, aged three years, three punctures were made to each of which was applied a tube of vaccine lymph removed from a healthy child rather more than a week previously. The vesicles on the child were very large and full, the lymph very fluid and abundant, and the areola more extensive and indurated than I generally meet with. On the second day after the operation all three spots felt elevated and firm, but there was no discoloration. On the fifth day one spot only could be distinguished. It was red, more elevated, of the size of half a pea, and possessed a minute brown crust on its apex. Two days later this vesicle appeared very red, was larger, and had a small brown apex; but there was no appearance of lymph, and no areola: nor was the udder hot and painful. On the ninth day this vesicle was larger, filled with a dirty yellowish fluid, surrounded by an areola without any subcutaneous induration, and covered with a large, dark-brown crust. From this date onwards desiccation gradually took place. The crust separated at the end of the third week after vaccination, and left a circular, pinkish, depressed scar.

A second carefully selected milch cow, about four years old, was vaccinated by punctures to which was applied lymph removed from a fine healthy child a month previously. A tube of lymph was used for each spot by blowing the lymph (as before) on the grooved needle and penetrating the skin thoroughly till a very minute drop of blood oozed out. On the second day after the operation both punctures in this cow were felt raised and firm; but there was no discoloration. On the fifth day one vesicle only could be distin-

[1] Ceely, Rob.: "Observations on the Variolæ Vaccinæ," in the 'Transactions of the Prov. Med. and Surg. Assoc.,' vol. viii, pp. 354 et seq.

guished. It was not so well formed as that on the last cow, but had a larger black crust as if it had been abraded. Two days later (seventh day) this vesicle was better developed, and was covered by a large dark-brown crust. The udder was hot, and the neighbourhood of the vesicle was somewhat painful. On the ninth day after vaccination all activity appeared to have ceased in this vesicle, and a black flattened crust indicated the seat of vaccination. The crust gradually contracted and separated about the fifteenth day, leaving a flattened, pinkish, circular mark. The health of neither of these cows was in any way affected, and they continued to furnish the ordinary amount of milk.

These experiments satisfied me that the inoculation of milking cows with humanised vaccine was more likely to be successful than was that of cows which were not yielding milk. A similar observation was made by Dr. Thiele of Kasan when he experimented by inoculating cows with the lymph of human smallpox.[1] This is probably owing to the increased vascular excitement and consequent local activity which accompany the secretion of milk. On the other hand in neither class of cases can we be at all sure of even a moderate amount of success. "Generally," says Dr. Seaton, "when the operation (of retrovaccination) is successful, there are early local symptoms, and the vesicle begins to form by the third or fourth day, and is distinctly formed, so as to yield lymph for use by the seventh; occasionally very little result of any kind is perceptible before the seventh day, and no evidence of the presence of lymph can be got before the ninth day; but, in either case, the vesicles reach their acme by the tenth day and then decline, the detachment of the crusts taking place from the seventeenth or eighteenth to the twenty-third or twenty-fourth day."

Having thus become satisfied that humanised vaccine succeeded in a certain proportion of instances in producing vaccinia in cows, especially in milch cows; but feeling assured that this mode of procuring lymph can never be of general avail because of the obstreperous nature of the animal, I next directed my attention to the vaccination of calves with humanised vaccine. Much better results have followed my vaccinations of calves with humanised lymph, and the application of this retrovaccine lymph to children. The vaccination of calves with humanised lymph has with me always been successful, though the vesicles thus induced did not, at first, equal the number of punctures so often as when original cowpox vaccine was inoculated into the heifer. Some of the finest vesicles I have met with were obtained by the vaccination of children with retrovaccine lymph from the heifer.

[1] 'Die Menschen-und Kuhpocken in ihrer Identität und Ruckbildung ersterer zur Vaccine,' &c., Von Dr. Basil Thiele, in Henke's 'Zeitschrift,' Erlangen, 1839, p. 26.

June 15th, 1870, vaccinated a heifer, ten weeks old, with lymph sixteen days old on the right side, and with lymph three months old on the left side of the abdomen; making twelve sets of scratches on each half of the belly. Two days later eight of each set of scratches were found raised, but unaccompanied by redness, heat, or tenderness of the skin. On the sixth day after vaccination two slight tubercles were observed on the right half of the abdomen, from one of which a child was vaccinated without success. The vaccinia then subsided without apparent scab formation. This and the succeeding heifers were vaccinated in the manner described by me in the 'Brit. and For. Med.-Chir. Rev.,' April, 1870, pp. 511 et seq.

June 21st, vaccinated another heifer (B) with six sets of scratches on either half of the abdomen; to those on the right half lymph three months old was applied, while to those on the left half lymph of the same date but from another child, was used. The heifer was six months old.

Vaccinated at the same time a third heifer (C), three months old, making also twelve sets of scratches. The six on the right half received lymph removed on the preceding day; to those on the left half was applied lymph about fourteen weeks old.

On the fourth day after the operation two sets of scratches on either half of B were slightly elevated but abraded; while four sets of scratches on either half of C were in a like condition.

Next day (fifth) B showed four raised but abraded spots; C showed only two. From B an adult was revaccinated successfully.

On the ninth day, four vesicles were seen on the left half of B, and six on the right half, each as large as a sixpence, on a firm base, desiccating, covered by a brownish crust, and containing scarcely any fluid. From one of these the preceding child was again vaccinated and with success. On the right side of C four vesicles appeared, but none on the left. Desiccation had commenced, and no lymph was obtainable. In other instances than the above I vacciuated both cows and heifers without recording the results, owing to want of leisure.

These experiments led me to believe that with due attention to certain minutiæ a cow or a heifer may be vaccinated as successfully as a child; that the form of lymph does not materially affect the result provided the lymph has been recently removed; that vaccination is more successful in the case of milch cows than in those not yielding milk, but that a heifer is preferable because more easily controlled; and that the season of the year influences but little the issue of the operation.

When able again to record the results of vaccinations of heifers, I paid special attention to those minute details which seemed to affect the success of my experiments, and as the following notes will show my former conclusions proved to be correct.

Feb. 7th, 1871, at the request of my friend, Mr. Bell of New

Brighton, I vaccinated, with his assistance, a calf of which he kindly took care. The animal was a fine, healthy male, about two months old. After shaving the lower half of the abdomen we vaccinated it in 120 spots, making both scratches and incisions, and applied to these fifteen tubes of lymph which had been removed on the previous day from two healthy children. In this instance all the points to which the lymph was applied yielded vesicles; and lymph was taken from these on the fifth, sixth, and seventh days by Mr. Bell and myself, and used for vaccinating and revaccinating human subjects. The vaccinia pursued its usual course in the case of this calf and of those referred to in the following table. The scratches or incisions were felt elevated and firm on the second or third day after the operation; on the fifth day, they were more raised, vesicular, and with a slightly red circumference; the vesicles had attained perfection on the seventh or eighth day, when they appeared full of lymph and showed a slight depression in their centre; next day they were covered by a brown crust; desiccation gradually progressed, and the crust fell off on the eighteenth to the twenty-first day. Lymph was removed for the most part on the fifth, sixth, or seventh days; but it proved sometimes active when taken as early as the fourth day, or as late as the ninth day.

Table showing the results of *retrovaccine* lymph—

February 7th, 1872, vaccinated heifer				A.
March 1st,	„	„	„	B.
„ 28th,	„	„	„	C.
May 23rd,	„	„	„	D.
June 6th,	„	„	„	E.
„ 21st	„	„	„	F.

The lymph from these heifers was used in the following cases, with these results:

From A, on the 5th day,	re-vaccination	primary	— V2	for two	sets of	scratches.	
„ 6th	„	„	— V2	„	„	„	
„ „	vaccination	„	— V3	three	„	„	
„ „	re-vaccination	„	— V2	two	„	„	
„ „	„	„	— V2	„	„	„	
„ „	„	„	— V1	„	„	„	
„ „	vaccination	„	— V3	three	„	„	

In these last five instances the lymph was used on the day after its removal from the heifer.

From B, on the 5th day,	revaccination	primary	— V3	for three	sets of	scratches.
„ „		„	— V3	„	„	„
„ „		„	— V3	„	„	„
„ „		„	— V3	„	„	„
„ „		„	— V3	„	„	„

had had variola when nine years old, *i.e.*, fifty years previously.

From B, on the 5th day, revaccination primary — V 3 for three sets of scratches had been successfully re-vaccinated 15 years previously, when ten years old.

From B, on the 5th day, re-vaccination secondary — V1 for two sets of scratches, the lymph was used in this case on the day after removal.

From B, on the 6th day, re-vaccination primary — V3 for three sets of scratches.
" " " — V1 two " "
" " " — failed; had been successfully re-vaccinated about thirteen years previously, when fifteen years old.
From B, on the 6th day, re-vaccination, primary — V2 for two sets of scratches.
" " secondary — V2 " "
" " " — V2 " "
" " " — V2 " "
when an infant had been vaccinated with spontaneous cowpox lymph.
From B, on the 6th day, re-vaccination secondary — failed.
" " " — failed; had had variola when thirteen years old.
From B, on the 6th day, revaccination secondary — V2 for two sets of scratches.
" " " — V2 " "
the lymph in this instance was used on the day after its removal.
From B, on the 7th day, vaccination primary — V4 for four sets of scratches.
" " " — V4 " "
" " " — V4 " "
" " " — V1 " "
" " " — V2 " "
" " re-vaccination primary — V3 " "
" " " " — failed.
" " " " — failed; was successfully re-vaccinated seven years previously.
From B, on the 7th day, re-vaccination primary — failed; had been successfully re-vaccinated twelve years before.
From B, on the 7th day, revaccination primary — failed.
" " " secondary — failed; was successfully re-vaccinated eight years previously.
From B, on the 7th day, re-vaccination secondary — V3 for three sets of scratches
" " primary — failed.
" " " — V4 for four sets of scratches.
" " " — V2 " two "
" " secondary — failed.
" " " — failed.
" " " — failed.
" " primary — V1 for three sets of scratches.
" " " — failed.
" " " — failed; had been successfully re-vaccinated twelve years before.
From B, on the 7th day, revaccination primary — failed; was successfully re-vaccinated eighteen years previously.
From B, on the 7th day, revaccination secondary — failed.
" " " — failed.
" " " — failed.

This same lymph I used several times during the following week but without success, and revaccinated the persons with ordinary lymph also without success. On the other hand I employed this

* In these last six cases the lymph was used on the day after its removal; in the succeeding six instances, the lymph was used two days after its removal; in the next five cases the lymph was used on the third day after its removal; and in the last two cases of this series on the fourth day after its removal from the heifer. By primary re-vaccination is meant that this was the first time the person had been re-vaccinated by me: while the secondary re-vaccinations were those instances in which I had first re-vaccinated with humanised vaccine, and failed.

lymph, fourteen days after its removal, on a person who had been previously revaccinated unsuccessfully with humanised vaccine, and obtained the full complement of vesicles for the scratches made.

From D, on the 6th day, re-vaccination, primary — V2 for two sets of scratches.
" " " — V2 " " "
" 7th " — V1 " " "

The lymph from E was sent abroad. With sixth day lymph from F I vaccinated two children successfully, on the third and fourth days after the removal of the lymph from the heifer.

Dr. Seaton states in his 'Handbook of Vaccination' (p. 97) that the transference of humanised lymph to the cow causes it to lose a certain amount of its potency, so that it requires to be transmitted through three or four generations of human subjects in order "to give it the same activity as it had before it was transferred." From the preceding notes, however, it will be observed that humanised vaccine thus transmitted through *healthy young* heifers was equally successful with ordinary lymph in producing the full complement of vesicles to the scratches made; and that such lymph frequently succeeded when humanised lymph failed. This increased activity of retrovaccine lymph over ordinary vaccine struck me so forcibly that at an early stage of these experiments I concluded that where retrovaccine lymph failed humanised vaccine would not succeed; and such proved to be the case in the several instances in which it was tested.

It is to be regretted that greater advantage was not taken of the admirable opportunity which presented itself in 1865-66, when numberless cows were vaccinated as a protection against the rinderpest. Facilities like those afforded by that vaccination panic are rarely met with, and many controverted questions might then have been readily settled.

From the previous observations it may be inferred that when recently removed humanised vaccine is applied with care in the inoculation of a healthy heifer perfect success will follow, both as regards the number of vesicles and their size and fulness. The want of success attendant on my first experiments was due to the restless nature of the animals handled. The cows and heifers I first experimented on were allowed to roam at large, and accordingly by abrading the vesicles they prevented their further development. In order to ensure success the heifer should be tied up after being vaccinated, so that she may not injure the vesicles. The preceding table also shows that retrovaccine lymph, like that of spontaneous cowpox and that derived by animal vaccination, cannot be preserved in an active state for so long a time as ordinary vaccine.

Lastly, retrovaccination is to be recommended as a ready means of multiplying the supply of lymph. It has been shown that on a heifer 120 vesicles may be produced by the employment of fifteen tubes of vaccine lymph, that from each of these vesicles three or

four persons can be vaccinated, that this lymph can be removed on three separate days (fifth, sixth, and seventh),[1] that this lymph can be obtained earlier than from children, and that retrovaccine lymph appears to act with more certainty than lymph which has been transmitted through human subjects for a long series of years. Some vaccinators also regard retrovaccine lymph as regenerated vaccine. Retrovaccine lymph is more readily obtained than vaccine derived by "animal vaccination."

[1] Drs. Carganico and Numann removed lymph on the eighth and ninth days after vaccination.

II. — Contributions to the Therapeutics of Diabetes Mellitus.

By Balthazar Foster, M.D., M.R.C.P. Lond., Physician to the General Hospital, and Professor of Medicine in Queen's College, Birmingham.

The cases which form this paper have been selected from a number in which I have made observations to determine the effect of remedies in controlling the excretion of sugar in diabetes mellitus. An inquiry of this kind is very laborious, and often productive of results quite incommensurate with the labour expended. The results, however, if accurately obtained, are not valueless, even when they add little or nothing to our curative power over the disease, for they increase our stock of knowledge, and they often indicate both the directions to be avoided and those to be followed in future research. Diabetes, unfortunately, is not the only pathological state for which we want to know what treatment is worthless, and what is valuable. The cases now recorded may possibly contain some indications of both kinds. To estimate the effects of any remedy on the sugar-excretion in diabetes, we should determine the amount of sugar lost under ordinary mixed diet, and under restricted diet containing little or no saccharine or farinaceous elements. When these two amounts are known, we are in a position to estimate the effect of treatment by drugs. In most of these cases this has been done, and in the few in which it could not be done the results are, nevertheless, trustworthy. Instead of giving daily records of the quantity and quality of the urinary secretion, I have recorded the results in the form of the *average* daily excretion for the period of each kind of treatment. This has been done for the sake of brevity, and also because diagrams have been made from time to time of the progress of each case, and it is difficult to illustrate in a diagram of convenient size the daily alterations in the quantity of water and sugar for a period of several months. I am aware that this method of recording the average daily excretion is open to some objections, the chief of which, however, is that it represents the effect of treatment in a much less striking way than a daily record which shows the progressive diminution of sugar and urine. Daily analyses of the urine were made in nearly every case, and in the exceptions they were made three or four times weekly. In all cases the urine of twenty-four hours was measured into a single vessel, and the specimen taken thence was examined for its specific gravity, and sugar contents.

The following drugs were given, and after each drug are stated the conclusions which the study of the cases seems to justify.

The peroxide of hydrogen was given in two cases. It had little or no influence in diminishing the sugar excretion. In the one case in which the sugar and water fell, the skin acted most copiously, and the perspiration contained a large quantity of sugar. The improvement in the state of the urine was, no doubt, due to this second channel of elimination. In other cases, not here recorded, the remedy has failed me.

The liquid extract of ergot was given in four cases, one of which was complicated by extensive lung and kidney mischief. The ergot was given on the hypothesis that it might lessen the congestion of the abdominal viscera. The effects observed to follow the use of ergot were—(1) decrease in the quantity of urinary water, (2) slight decrease in the quantity of sugar, (3) slight fall in the specific gravity of the urine. The decrease in urinary water I have also observed in cases of diabetes insipidus, as well as in other cases of true diabetes.

The potash salts were given in succession to the same patient. The results show that they have no power of diminishing the quantity of urine or the amount of sugar. In other cases I have obtained similar results with alkalies, and have never observed any beneficial effects from their use in diabetes. It is worthy of note that in neither case was the acid reaction of the urine neutralised, although the urine is twice noted as being only faintly acid.

Opium was given to the same patient after the potash salts. The quantity of water fell, the specific gravity remained very stationary, and the quantity of sugar was lessened. The patient, however, improved manifestly out of proportion to the diminution of the sugar, an improvement which I have noticed in other cases under opium, when the sugar diminution has not been great, and which I am inclined to attribute to the influence of the opium in lessening the urea excreted. In the case here recorded the urea fell in the course of ten days nearly 120 grains. The opium in this case did not act nearly as well as it does in some cases.

Bromide of potassium in combination with Tr. Ferri Perchloridi was given in two cases. This combination I have found much more useful in diabetes than the bromide alone; I have, however, only found it beneficial in mild cases. In such cases it seems to have some slight influence in diminishing the amount of water and checking the craving after food. This latter is its chief recommendation.

Lactic acid was given in four of the cases, and in one case combined with skim milk. In three of the cases the lactic acid was given in small doses, not exceeding two drachms a day. In two of these, rheumatic symptoms were produced, the quantity of urine fell, the specific gravity was not much affected, the sugar was diminished in quantity and the patients improved. In one case the

sugar was not estimated, but the quantity of urine was lessened, and the specific gravity fell slightly, while no rheumatic symptoms appeared. In one of the cases the lactic acid was given in much larger doses to the extent of 3 oz. daily. In this case no rheumatic symptoms whatever were produced. The urinary water was diminished, the specific gravity was scarcely affected, and the sugar fell slightly, rising again when the acid was discontinued. In all these cases but one the functions of the skin were restored by the lactic acid. In this way the decrease of water may be wholly or partly explained. Thirst was not diminished particularly, and the appetite was if anything increased.

The use of lactic acid in cases dieted on skim milk is illustrated by one case. In mild forms of diabetes this treatment has often succeeded in my hands. Its chief recommendation is its greater ease of application than restriction to purely non-amylaceous food.

In addition to the effect of remedies, the cases which follow illustrate some other points of interest—*e.g.* the frequency of grief as an antecedent of the disease; the frail tenure on which the life of a diabetic is held, and the slight exertion by which this tenure is destroyed; and lastly the congestion of the abdominal viscera is exemplified by the post-mortem appearances in Case 5.

Case 1.—*Treated with peroxide of hydrogen.*

John S—, æt. 29, iron-roller, three children; tall, sandy hair, complexion rather florid. Family history good; always healthy himself till about two months before admission, when he felt weak and had pains in his limbs after work. He noticed that his thirst was great, and his water increased in quantity. He considers that his illness was brought on by worry over family troubles. He passed at one time as much as six pints of water in the night. Has lost twenty-one pounds weight since he began to be ill. On examination his lungs and heart were found to be healthy. Tongue was red at tip and edges, white at base, and rather dry. Gums rather spongy, teeth sound. Skin dry and harsh. He complained of pain in frontal region of head, which he says has troubled him all through his illness. Weight 8st. 12lbs. Temperature of body 96⅗° to 97⅘°, once as low as 94⅖°; during whole of stay in hospital it was never once above normal line. Progress as follows:—

Ounces of urine in 24 hours.	Sp. gr.	Grains of sugar per oz.	Daily amount of sugar in grains.	Remarks, diet, treatment.
217	1037	36·8	7985·6	Average of 7 days on ordinary mixed diet.
128	1040	42	5376	Average of 7 days, partially restricted diet, *i. e.* bran bread, green vegetables, meat, milk; ʒss Tr. Ferri Perch., daily.
101	1038	38	3838	Average of 7 days, diet as before, skin softer; peroxide of hydrogen, ʒij daily.
84	1028	23	1932	Average of 9 days, restricted diet, *i. e.* no bran bread; ʒiij of peroxide of hydrogen daily; copious perspirations.

This patient remained some weeks longer in the hospital, but as he could not be kept wholly on starchless food he was allowed Blatchley's bran-cakes. At the time of his discharge he was passing 75 oz. of water (on on average) containing 22 grs. of sugar per oz. He continued the peroxide of hydroden which he said did him more good than anything. While under the influence of this drug he perspired copiously, and the perspiration contained sugar in considerable quantity—on one occasion as much as 8 grs. per oz. The perspirations were so free that the secretion could be easily collected in a test tube. He gained while under treatment 3 lbs. in weight.

CASE 2.—*Treated with* (1) *liquid extract of ergot and* (2) *peroxide of hydrogen.*

Elizabeth J—, æt. 40, governess, florid complexion, sandy hair. Family history good. Always enjoyed good health till some twelve months ago, when she was about to be married. A week before the wedding-day her intended husband was thrown from his horse and killed. She was severely affected by the accident, and for some weeks confined to her bed. On getting about again she noticed that her strength did not return, and that she suffered from obscure pains in the back and in the head. About three months later she noticed that she passed an unusual quantity of water. Her appetite was good and thirst great. She lost flesh, and at the time of her admission to the hospital weighed only 7 st. 2 lbs., having lost one and a half stone. Skin was dry; temperature 97⅓°; tongue red and fissured; expression anxious; temper irritable. Heart and lungs free from disease. Appetite voracious. Bowels confined. After being under treatment for nine weeks she left the hospital, having gained 2 lbs. in weight. She died afterwards of diabetic phthisis. On admission she was placed on ordinary diet for four days. The report of urine was as follows:

Ounces of urine in 24 hours.	Sp. gr.	Grains of sugar per oz.	Daily amount of sugar in grains.	Remarks, diet, treatment.
185	1032	25·5	4717·5	Average of 4 days on ordinary mixed diet.
160	1030	24	3840	Average of 4 days on partially restricted diet, *i. e.* meat, milk, green vegetables, and bran bread.
135	1027	23·7	3199·5	Average of 10 days on same diet; Liq. Ergotæ ʒiss in day.
170	1030	19	3230	Average of 8 days on same diet; no ergot.
116	1027	16·25	1885	Average of 5 days on same diet; Liq. Ergotæ ʒiij a day.
At this period the ergot was stopped on account of pains in lower limbs and general malaise, the water increased the next day to 130 oz., but the sugar fell for two days till it reached only 11 grs. per oz. Peroxide of hydrogen was ordered with the following results:				
120	1028	20	2400	Diet as before; peroxide of hydrogen ʒiij a day; average of 4 days; skin dry.
156	1035	29·7	4633·2	Ordinary mixed diet. Patient helped herself to white bread. Peroxide of hydrogen as before. Skin dry. Average of 3 days.
126	1034	20·6	2595·6	Partially restricted diet; peroxide of hydrogen ʒv a day; average of 7 days; skin dry.

CASE 3.—*Treated with* (1) *Ext. Ergotæ Liq.*, (2) *bicarbonate of potash*, (3) *citrate of potash*, (4) *opium.*

Alfred S—, æt. 22, miner, florid complexion, lightish brown hair. Family history good. Was always strong and healthy till seventeen months before his admission to the hospital, when he had a bad attack of typhoid fever, and relapsed in consequence of getting up too soon. He never regained his strength, and always felt thirsty. During his convalescence he noticed that he passed a large quantity of water. He got thinner, in spite of a large appetite; bowels always troublesome; skin very dry and rough. On admission his lungs and heart were found to be free from disease; tongue furred. Morning temperature second day after admission 96° F. Weight 8 st. 8½ lbs. This man left the hospital at the end of seven weeks, having improved a good deal under the opium treatment, which, however, could not be pushed beyond sixteen grains daily, on account of head symptoms. When he left the hospital he weighed 9 st. 1½ lb. He continued the opium treatment for some time, and improved considerably, passing generally under 100 ounces of water, with sometimes as little as twenty-one grains of sugar per ounce. I have lost

the table of analyses made at this period, so have not been able to include results in the table.

Ounces of urine in 24 hours.	Sp. gr.	Grains of sugar per oz.	Daily amount of sugar in grains.	Remarks, diet, treatment.
220	1042	41	9020	Average of 3 days on ordinary mixed diet.
164	1038	35	5740	10 days average on restricted diet.
124·5	1042	41·8	5204·1	7 days average, restricted diet; ℥iiss Liq. Ergotæ daily.
134	1039	46·5	6231	7 days average, restricted diet; bicarbonate of potash ℥vj, a day; urine acid, but on 2 days only slightly so.
141	1038	44	6204	6 days average, restricted diet; ℥iss of citrate of potash daily; severe purging produced after 6th day, so medicine was stopped; urine never lost its acid reaction.
132	1038	47·1	6217·2	Average of 7 days, restricted diet; 5 grs. of opium a day.
118	1039	40·8	4814·4	Average of 7 days, restricted diet; 12 grs. of opium a day.
124	1039	42·2	5232·8	Average of 7 days, restricted diet, 6 oz. of white bread; 15 to 16 grs. of opium a day.

In this case the urea was estimated five times during the last ten days of the opium treatment. On the first occasion it was 820·15 grains per day; then it fell as follows:—730·08 grains, 645·12 grains, 713·16 grains, 701·8 grains, per day. The last two estimates were made while he was taking white bread extra.

Case 4.—*Treated with liquid extract of ergot.*

Sarah A. H—, æt. 22, plate-glass smoother, florid complexion, dark hair. Family history good. Father, who suffered from chronic rheumatism, died about eighteen months ago. She was very greatly distressed at her father's death, and has never felt well since, being low spirited, and not caring to mix with any of her friends or companions. Illness began by pain in loins, especially on left side. When at work she was easily fatigued, and when tired the pain in the back increased, and pain in the head came on. She was very hungry and thirsty at this time, and drank freely of water and ginger beer. She passed water very frequently, and had a good deal of itching about the genitals. Menstrual functions regular, but discharge scanty and like dirty water. About two months before admission she began to lose flesh rapidly; her fingers gathered about this time, and she lost several of her nails. She was treated at home for two or three months, but applied at the hospital, as she became worse. On admission (May 23rd, 1869) her thoracic organs were healthy;

liver somewhat enlarged. Tenderness on pressure over left kidney region. Tongue red, with pale fur at base. Appetite excessive. Bowels very obstinate. Skin dry and branny. Passes 320 to 360 ounces of water in twenty-four hours. Reports of urine and results of treatment as follows:

Ounces of urine in 24 hours.	Sp. gr.	Grains of sugar per oz.	Daily amount of sugar in grains.	Remarks, diet, treatment.
310	1040	50	15500	Average of 3 days on ordinary mixed died; temperature 96·5° to 97·2°.
262	1043	48	12576	Average of 7 days, partially restricted diet, *i. e.* bran bread, meat, milk, green vegetables.
230	1044	49	11270	Average of 7 days, diet as before; Liq. Ergotæ ʒiss, daily.
180	1040	38	6840	Average of 7 days, diet as before; Liq. Ergotæ ʒiij, daily.

The ergot was stopped after this, as the patient complained of great pains in abdomen and in legs, and general malaise. After this she got restless and left the hospital, wishing to be with her friends. The case was a most severe one, and illustrates the effect of ergot in diminishing the quantity of water. The fall in the amount of sugar was considerable, I think greater than the restriction of diet would account for.

CASE 5.—*Diabetes complicated with albuminuria and pneumonia, &c.; treated with ergot.*

Jane F—, æt. 27, laundress. No family history of diabetes. She had peritonitis and variola six years ago, and has never been as strong as before the latter illness. About three years ago she began to pass an excessive quantity of water; her appetite was hearty, but not voracious; thirst great. Emaciation began soon, and she has lost flesh ever since. About twelve months before admission to the hospital she caught cold, her legs swelled, and she noticed that she was puffy under the eyes; she also had a distressing cough. She has been unable to work for some months, and has for some weeks been confined to her bed. When admitted (November 1st, 1870) she was greatly emaciated, face puffy, legs œdematous, tongue moist and red, bowels confined. Heart sounds healthy. Upper lobe of left lung dull on percussion, both in front and behind. Under middle third of left clavicle was detected a cavity with fluid contents; in its neighbourhood were heard coarse mucous râles, and lower down bronchial respiration. Chest flattened on left side. Right apex dull on percussion, with harsh breathing, almost bronchial, and

prolonged expiration. Liver enlarged, projecting about an inch and a half below costal arch. Urine 130 oz., sp. gr. 1040; *neutral reaction* contained a large quantity of sugar, and about $\frac{1}{8}$th albumen and a few epithelial casts. Temperature 97$\frac{2}{5}$°. She was placed upon milk diet, and ordered Ext. Ergotæ Liq. ♏x ter die. During the next week the urine fell in quantity as follows:—110, 112, 60, 96, 106, 80, and contained from 35 to 46 grs. of sugar per oz., the specific gravity remaining nearly stationary. Her temperature fell as low as 95$\frac{4}{5}$° on several occasions in the morning, but began, about November 9th, to rise as high as 98° in the evening. The softening in the left upper lobe went on extending, but in other respects she appeared better. On November 16th her morning temperature was as low as 94$\frac{2}{5}$°. The urine had fallen to 90 oz., sp. gr. 1040, with 29$\frac{1}{2}$ grs. of sugar per oz., or 2655 grs. in twenty-four hours. At this time she was taking ʒiss of ergot daily, and in addition to four pints of milk she consumed 8 oz. of bran bread, one egg, and a chop. In the evening of this day fine crepitation was detected at left mamma, and the dulness was found to have extended nearly to base of left lung in front. The softening at right apex was advancing rapidly. The temperature rose at 10 p.m. to 100$\frac{2}{5}$°, or six degrees Fahr. above the morning temperature. After this she rapidly grew worse, and the ergot was discontinued. Her temperature only once fell below 98°, and in the evening varied from 99$\frac{1}{5}$° to 100°. On the evening of the 21st it reached to 101$\frac{4}{5}$°. The albumen had increased on this day to $\frac{1}{3}$ col., and the sugar was 50 grs. per oz.; the water, however, had fallen to 50 oz. On the 28th she was seized with a severe pain below the left nipple and great dyspnœa; on deep inspiration a friction sound was heard. She gradually became comatose, and died the next morning.

The notes of the post-mortem examination were condensed as follows, by my clinical clerk (Mr. E. A. Elkington, M.B.):—Rigor mortis well marked, muscles of a fair colour, though a little pale; free from any fatty change. On opening the abdomen the liver was seen to greatly exceed its normal limits, especially towards the left side. The liver was universally congested especially the hepatic system, the capsule was rather opaque and adherent in some places. The gall-bladder was small and contained only a little brownish bile. The liver weighed sixty-eight ounces. *The peritoneum covering the intestine and forming the omentum was seen to be minutely injected, the small capillaries being filled with red blood.* There were no adhesions connected with the peritoneum, and no fluid in the cavity. The spleen was healthy in appearance and weighed six ounces, the kidneys nine ounces each. The capsule was easily separable from each, leaving the organ lobulated, with the cortical substance granular to the touch and like dried orange-peel. Over the surface of the organs were numerous branching tortuous veins, the pyramids

appeared healthy, the cortex was hard, tough, and granular. The bladder was much hypertrophied and dilated, capable when empty of holding two oranges. Uterus small, weighing with ovaries one and a half ounces.

The left pleural cavity contained fully a pint of semi-opaque serum with flakes of lymph; there were numerous recent adhesions; the left lung was smeared over with recent lymph, which glued it to the pericardium on one side, and to the diaphragm below. No fluid in right pleural cavity. The left lung when placed in water floated with the base uppermost, its apex was riddled with cavities mostly about the size of a marble; three fourths of the lung at least was infiltrated with grey tubercular (?) matter, which in many spots had softened and was converted into small purulent depôts. The right lung was in a similar state as regards its apex. There were deposits of yellow cheesy matter, grey tubercular and calcareous deposits all through it, and one good sized vomica at the base. There was a little bloody serum in the pericardium; the heart was healthy.

The fluid contained in the left pleural cavity was highly albuminous *and contained two and a half grains of sugar per ounce.*

A decoction of half an ounce of the liver substance in water showed only a trace of sugar.

CASE 6. *Treated with* (1) *Tr. Ferri Perchlor. and bromide of potassium* (2) *with skim milk.*

Mr. W. R. W—, 30 years, tall, dark complexion, slight. Has lived in Australia, whence he came to England three and half years ago. Always enjoyed good, though not strong health till about three months ago, when he received a severe blow over the last two dorsal vertebræ. He did not take much notice of the injury at the time, but has frequently had dull pain there since. Illness began with feeling of languor, considerable thirst, and large secretion of urine. Appetite large, but steady loss of flesh; bowels inactive.

On examination no trace of injury could be found on back; there was tenderness on pressure over left kidney; lungs and heart healthy; liver and spleen normal in size; tongue red, moist, and fissured; gums spongy; right molar teeth carious; skin dry and hard; urine has been as much as five pints daily, is now less. Progress of case was as follows:—

Ounces of urine in 24 hours.	Sp. gr.	Grains of sugar per oz.	Daily amount of sugar in grains.	Remarks, diet, treatment.
76	1038	30	2280	Average of 3 days on partially restricted diet.
64	1035	24·5	1568	Average of 1 week, diet partially restricted; taking Tr. Ferri Perchlor. 3ss and bromide of potassium 40 grs., daily.
42	1040	15	630	Average of 1 week; same medicine; strict diet.
40	1030	3	120	Average of 1 week; same medicine; skin moist and perspiring; strict diet.
38	1029	Trace	...	Two days average, strict diet, same medicine. Between this time and the previous observation, an attempt to take some white bread on one day, as part of diet had caused the sugar to rise to 12 grs. per oz., the water to 57 oz.
Four months later Mr. W— came back to me with all his old symptoms. A return to ordinary mixed diet had caused the relapse. He passed at this time—				
72	1037	21	1512	3 days average on mixed diet.
47	1035	18	846	1st week, daily average, skim-milk treatment.
56	1029	7½	420	2nd week, daily average, skim-milk.

After this he improved still more, but I had no opportunity of testing the disease. Later on he again relapsed, and I am informed that he died in about twelve months after last observation.

Case 7.—*Treated with lactic acid.*

John W—, æt. 31, married, several children, healthy, ironcaster; reddish hair, complexion fresh. Family history good. Had always enjoyed good health up to the present illness, never had rheumatism in any form. Began to feel low and weak some four months before he came to the hospital. Felt unfit for work, had frequently to pass water and was very thirsty; appetite was very good all through illness and has increased of late. He nevertheless has lost flesh to the extent of nearly 2 stones, weighing now only 7 st. 5½ lbs. Tongue was red and fissured, mouth parched. Gums spongy; teeth, with one exception, sound. Bowels confined. Liver not enlarged. Lung percussion good everywhere, and no abnormal sounds on auscultation. Respiration 18 per minute; pulse 72. Heart sounds healthy. Temperature of body 96⅘°. Urine copious, free from albumen, contains chlorides and a large quantity of sugar. Skin dry and branny. Progress of case was as follows:—

Ounces of urine in 24 hours.	Sp. gr.	Grains of sugar per oz.	Daily amount of sugar in grains.	Remarks, diet, treatment.
180	1047	46	8320	Average of 7 days, ordinary mixed diet; no medicine.
116	1042	36	4176	Average of 14 days, restricted diet; no medicine.
About the middle of this period the lung complications began to manifest themselves at right apex, as pneumonia.				
117	1041	27·5	3217.5	Average of 7 days, restricted diet; lactic acid 45 to 90 minims daily; acid taken irregularly on account of first attack of rheumatism; skin moist and perspiring.
123	1041	36	4428	Average of 14 days, diet as before. Recovering from rheumatic attack. No lactic acid; no drugs. Softening at right apex; free rusty expectoration.
119	1034	26·6	3165·4	Average of 6 days, diet as before; lactic acid 75 minims daily. The acid had to be stopped at end of 6 days, on account of rheumatism.
109	1038	24	2616	Average of 8 days, diet as before; lactic acid taken occasionally, about 50 minims a day. Two slight rheumatic attacks; skin perspiring freely.
Five weeks interval, during which lactic acid was taken irregularly, as patient liked, on account of rheumatism.				
103·6	1041	19·8	2051·3	June 1 to 15, daily average; diet as before; lactic acid 50 minims regularly; lung complications better; slight rheumatic attacks; skin perspiring freely.
97·4	1042	17	1655·8	June 16 to 30, daily average; diet as before; lactic acid 75 minims daily. Skin acting freely. Lung conditions improving much.
110	1041	28	3080	July 1 to 8, daily average; white bread 8 oz.; 75 minims 5 days; 100 minims of lactic acid, 3 days; and stopped on evening of 8th, on account of rheumatic attack. Lung complication stationary.
107	1043	31	3317	July 10 to 17, white bread as before. Sharp rheumatic attack. 100 minims of lactic acid on 10th only. Skin perspiring freely.
98	1041	19·2	1881·6	July 17 to Aug. 2, no white bread, restricted diet; lactic acid 100 to 125 minims daily. Skin acting freely; no rheumatism; lung conditions quiescent; harsh breathing at right apex; no expectoration.

Note.—Some two months after he left the hospital the lung complications set in again, and he died in three weeks. The lung affection while he was in the hospital was pneumonic in its character, and the expectoration was rusty; with advent of the lung disease the temperature rose and continued high till the last few weeks of his stay in the hospital, when it frequently fell to 97° in the morning, and in the evening was seldom over 98⅘°. During the attacks of joint inflammation the febrile elvation was greater, as I have elsewhere stated ('British Medical Journal,' Dec. 22rd, 1871. "The Synthesis of Acute Rheumatism"). During the time this man remained under treatment he gained weight in spite of the febrile attacks; in the middle of June he was six pounds heavier than at the time of admission (February 16th), and 9 lbs. heavier than he was early in March. He lost weight when he took to mixed diet again in July, but regained a little during the last seventeen days he remained under treatment.

CASE 8.—*Treated with* (1) *perchloride of iron and bromide of potassium and* (2) *lactic acid.*

Mr. John T—, æt. 37, gunmaker; married, two children; sandy hair; rather sallow complexion; slightly made. Family history good. Was always healthy till recently. Never had rheumatism, nor any other illness except summer diarrhœa. Temperate. His present illness began some eight months ago when he was in a state of great mental distress at the loss of one of his children. He was troubled with great thirst, and passed water too freely. He was tired and unfit to attend to business, easily fatigued by walking, and lost flesh although his appetite was very good. His disease was recognised by his family doctor, and he was placed under treatment by diet, with the effect of diminishing the amount of water without improving his strength or general health. When he consulted me he complained mainly of great weakness and inability to attend to his business, increasing emaciation, excessive diuresis (150 to 200 oz). His expression was anxious, tongue red, mouth clammy, gums softish, teeth sound. Appetite large, but heaviness after meals. Bowels confined. Thoracic organs free from disease. Liver and spleen normal in size. Skin dry and harsh. Pulse 78; respirations 18. Report of urine was as follows:—

Ounces of urine in 24 hours.	Sp. gr.	Grains of sugar per oz.	Daily amount of sugar in grains.	Remarks, diet, treatment.
160	1040	21	3360	Average of 3 days, ordinary mixed diet; Tr. Ferri Perch. ♏x t. d. s.
79	1034	18	1422	Average of 7 days, restricted diet, *i. e.* meat, fish, eggs, bran bread, water-cress, milk 1 pint, claret 1 pint; Pot. Brom. gr. xx, Tr. Ferri Perch. ♏x, t. d. s., Ol. Morrhuæ ʒss bis die.
70	1038	22	1540	Average of 4 days, restricted diet as before, + 2 pints of milk and half a pint of claret; lactic acid 50 minims a day; skin moist.
80	1038	14·8	1184	Interval of 2 weeks; during which the lactic acid was taken irregularly on account of rheumatic pains. The figures are average of 3 days in middle of this period.
74	1038	14·5	1073	Average of 1 week, restricted diet as above; 75 minims of lactic daily; skin moist and perspiring; acid stopped on account of rheumatic attack.
68	1036	12·5	850	Average of 14 days, 18 days interval having elapsed since last series of observations; restricted diet, gluten bread, skim-milk ʒiss of lactic acid daily for 9 days, ʒij daily for 5 days; no cod-liver oil; no claret.

Note.—After this period I did not see Mr. T— for some six weeks, during which, as I afterwards learned, he followed out the lactic acid treatment at intervals, but never took more than 120 minims in the course of twenty-four hours. He considered that he was improving; and, as I had advised, spent much of his time in the open air, going daily in an omnibus to the outside of the town, where he took a short walk into the country, and then returned home by the same conveyance. On the day but one before he sent for me, he had walked farther than usual into the country and had missed his return omnibus, and had in consequence walked home a distance of some four miles. He arrived in a state of extreme exhaustion. The next day he felt extremely weak and did not go out; he took some purgative medicine which did not act. I saw him the next day and found him in bed very ill, breathing at the rate of 45 per minute with a pulse of 110. Skin cold and dry. The lungs were clear all over, and were filled by each inspiration fully. No abnormal sounds except loud breathing everywhere, but still he seemed to be breathless; the inspirations were full and deep, but evidently ineffective. The peculiar breathing went on for twelve hours, when he gradually became drowsy and comatose, and died

eighteen hours after I saw him; during this period he had passed hardly any water.

Some time after, I saw a similar ending to a case of diabetes in a patient whom I saw in consultation with Mr. J. Jackson of this town. A young lady, æt. 29, who had suffered for some eighteen months, from diabetes brought on by mental anxiety, arranged to visit Birmingham in order to spend the Christmas with her friends. She felt quite as well as usual on the morning of her journey, and quite equal she thought to the effort. She had some distance to drive to the railway station, and then some twenty miles to travel. As usual at the period of the year the train was late, and she was fatigued by the extra time spent in reaching her friend's house. Mr. Jackson saw her that evening, and the next morning I visited the patient, whom I found in bed, feeling very exhausted and greatly distressed by a feeling of breathlessness. Long, deep, inspirations, which fully inflated the lungs (for they were free from disease) succeeded one another rapidly, but gave no relief. Her intellect was clear, and there was no blueness of the lips. In spite of stimulant remedies, peroxide of hydrogen, &c., she steadily grew worse and died some twelve hours after I saw her. She passed urine in small quantities for a diabetic during the day, and I found that it contained 25·7 grs. of sugar per oz.

CASE 9.—*Treated with lactid acid.*

George W—, labourer, æt. 52. Tall, thin; red hair; pale complexion, anxious expression, applied at the hospital suffering from diabetes. His family history was good, and he himself had always enjoyed fairly good health till some twelve months since, when he began to feel greatly fatigued at the end of his day's work, and to be troubled with great thirst. He soon began to lose flesh, and said he had lost 28 lbs. during the last six months. His appetite was good. Tongue red and fissured, mouth clammy, gums tender, teeth carious, breath sweet. He complained of sinking at stomach. Liver not enlarged. Lungs and heart free from disease. Pulse 66; skin dry and rough; temperature 97°. Urine copious, contained no albumen nor phosphates, but a large quantity of sugar. He was placed on ordinary diet at first, and the following was the progress of the case during his stay in the hospital: the amount of sugar was not estimated:

Ounces of urine in 24 hours.	Sp. gr.	Remarks, diet, treatment.
192	1039	Average of 3 days, ordinary diet; no drugs.
90	1035	Average of 7 days, restricted diet; no drugs.
71	1033	7 days average of restricted diet; ʒij lactic acid daily.
103	1035	Last 4 days in hospital, restricted diet; no lactic acid; no drugs.

CASE 10.—*Treated with lactic acid and skim-milk.*

Mr. W. T—, æt. 45, was sent to me by one of my colleagues to be treated for diabetes. Mr. T. is single, has always lived temperately and enjoyed good health up to about two years before he consulted me. He refers his diabetic symptoms to the mental distress from which he suffered at the time of his mother's death. Illness began with weariness, indigestion, thirst, and frequent micturition with increase in urinary secretion. His appetite continued good, but he has steadily lost flesh, being about 22 lbs. below ordinary weight. For eighteen months past he has lived on partially restricted diet consisting of meat, green vegetables, 6 oz. of toasted bread and claret. During last month has lost 1¼ lb. in weight. He complains of feeling chilly. Temperature 97⅘ F. Tongue furred, red edges; gums spongy, teeth decayed. Appetite not quite so good lately. Heart and lungs free from disease; liver not perceptibly enlarged. Skin dry and rough. Weight 8st. 10lbs. The following table shows the state of urine when he was first seen, and at the end of four weeks after he was placed on restricted diet and lactic acid. The figures in each line, except the first, represent the weekly average. During the month he gained 3 lbs. in weight. He has continued to improve I hear.

Ounces of urine in 24 hours.	Sp. gr.	Grains of sugar per oz.	Daily amount of sugar in grains.	Remarks, diet, treatment.
130	1033	25	3250	Three days average, partially restricted diet; no treatment.
120	1032	24	2880	Average for 1st week, restricted diet. 2 pints of skim-milk; lactic acid ʒj daily.
112	...	19	2128	Average for 2nd week, ditto.
90	1032	15	1350	Average for 3rd week, diet the same; ʒij of lactic acid in 24 hours.
84	1030	12	1008	Fourth week, ditto.

CASE 11.—*Treated with lactic acid.*

Samuel J—, æt. 39; three children; thin, slightly built, light hair, rather fresh complexion, anxious expression. Has served as a soldier, but was discharged on account of shortness of breath. Since then has worked as a lapidary and enjoyed fair health, except for colds on the chest, till some twelve months ago, when he began to feel stiff about the joints and very tired after his work. He noticed about the same time that he was always hungry and felt a sinking sensation at stomach very often. His thirst was also great and he passed water very freely. He knows no cause for the illness coming on. On examination his chest was found to be rather barrel-shaped, free from dullness, slight bronchitic râles both in front and behind. Cough, not very troublesome, much as it usually is he says. Tongue furred, gums spongy, teeth carious. Liver rather large coming nearly one inch below costal arch. Spleen natural in size. Skin dry and branny. No œdema of legs. Urine free from albumen, but contains a quantity of sugar. Specific gravity 1045. He was treated first as an out-patient and afterwards admitted into the hospital. While an out-patient he lost 6 lbs. in weight, in one month. It was found impossible to keep this man on thoroughly restricted diet, he helped himself constantly to the bread of other patients when it was attempted. Progress of case as follows:

Ounces of urine in 24 hours.	Sp. gr.	Grains of sugar per oz.	Daily amount of sugar in grains.	Remarks, diet, treatment.
172	1039	28·5	4902	Average of 4 days, diet partially restricted, *i. e.* skim-milk 4 to 5 pints, meat, 1 egg, bread 6 oz.; no medicine, except Ol. Morrhuæ ʒij t. d.
117	1038	26·8	3135·6	Average of 7 days, diet as above, except skim-milk 3 pints; lactic acid ʒij daily.
100·5	1040	23·8	2391·9	Average of 7 days, diet as above, except skim-milk 3 pints; lactic acid ʒj to ʒiss daily. Skin moist.
96	1039	26·5	2544	Average of 6 days, diet as above, except skim-milk 3 pints; lactic acid ʒij to ʒiij daily. Skin perspiring freely. On 6th day acid was stopped on account of purging.
117	1040	27·6	3229·2	Three days interval; average of 5 days, diet as above; no lactic acid. Gained 2¼ lbs. during whole period of treatment.

Note.—In estimating the effect of the lactic acid on the quantity of urinary water in this case, it must be remembered that a large

amount of fluid was taken as medicine, no less than 48 oz. in the twenty-four hours, during the period of the largest doses of lactic acid. The acid was dissolved in that quantity of water, and 2 to 4 oz. of the mixture taken every hour. In spite of this, the quantity of urine fell, but the functions of the skin became more active from day to day.

The lactic acid was given in the quantities mentioned in order to test the influence of large doses on the urinary excretion, and further, to determine, if large doses would develop rheumatic symptoms after the failure of small ones. The results show that the power of lactic acid over the quantity of urine and the sugar-impregnation does not increase in proportion to the dose, and that very large quantities of the drug may be given without the development of rheumatism. In this last respect the results confirm those obtained by other physicians, and especially by Dr. George W. Balfour. In several instances in which lactic acid has been given to my out-patients there has been no occurrence of rheumatic pains. The case of Wright (Case 7), however, was such a striking example of the manifestation of rheumatic symptoms over and over again under the use of lactic acid; the inflammation of the joints being perfectly under the control of the medicine, coming on when it was taken and gradually passing away when it was discontinued; that I can have no manner of doubt as to the influence of the acid in developing rheumatic fever under favourable conditions. It is to the exact definition of these favourable conditions that future inquiry must be directed in order to settle the pathology of acute rheumatism.

Chronicle of Medical Science.

REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.
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On the Use of Koumiss in the Treatment of Phthisis. By Dr. Victor Jagielski.—Dr. Jagielski, who has previously described the physiological properties of koumiss in several publications, offers some additional remarks on the subject. He states it as his opinion that the exemption of Tartary, Iceland, and Shetland from phthisis is not a mere coincidence, or a result of climatic influence, but is due to the general use of koumiss by the inhabitants of those places. The koumiss, or fermented milk, though made in some countries from mare's milk, and in others from cow's milk, has the same properties in all cases, and it might be made from any other milk, with suitable modifications, and would produce the same effect in a dietetic and therapeutical point of view. The substances called in Tartary "koumiss," in Shetland "bland," and in Iceland "syre," belong to the same ancient Scandinavian beverage, and whether the climate be hot, as in Tartary, or cold as in Iceland, its prophylactic or curative powers in relation to phthisis are, according to Dr. Jagielski, the same. Koumiss, when taken into the system, tends to produce fat in the subcutaneous areolar tissue in a comparatively short time, it improves the digestive powers, promotes the due assimilation of food, and prevents the decay and waste of the tissues; and it is said, moreover, as showing the relation existing between the use of koumiss and the exemption from consumption, that in proportion as this beverage has been discontinued in Shetland and tea and coffee substituted, so has phthisis increased. Koumiss, then, from its chemical as well as its therapeutical properties (for it contains a large quantity of casein and fat), is a most valuable agent in the treatment of consumption, and Dr. Jagielski specifies the different kinds of koumiss, arranged according to their respective chemical characters, which he recommends for different constitutions and for different stages of the disease.—*British Medical Journal*, Feb. 3rd, 1872.

On the value of Quinine as a Prophylactic. By Dr. J. B. Hamilton, Royal Artillery.—Dr. Hamilton has had opportunities in the West Indies, in Central America, and in the East Indies, of

testing the value of quinine and its allied drugs as prophylactics where malaria is present. In Jubbulpore, in the East Indies, Dr. Hamilton had the care of a battery of the Royal Artillery, and he gave the men under his charge two grains of quinine each with marked benefit, during the months of September and October, 1865. In 1866 a regiment of the Line arrived at the same station, and as the surgeon of the regiment did not believe in the prophylactic powers of quinine, the alkaloid was not administered to the men, although Dr. Hamilton continued to give it to his battery. The wet season of 1866 happened to be a very malarious one, and fever of a severe remittent type attacked the regiment, 150 men being in hospital out of a force of 500, and about twenty deaths took place. During the same time the health of Dr. Hamilton's battery remained remarkably good, only about 4 per cent. being in the hospital, and no deaths occurring. Ever since that year Dr. Hamilton has carried out the same plan, and last year he made some comparative experiments as to the value of quinine, cinchonine, and quinodine, and the result was that he placed cinchonine first as a prophylactic, then quinine, and at a long interval, quinodine. He finds that by the prophylactic plan the cases of fever are fewer, of a milder type, and more amenable to treatment, while the sequelæ, such as dysentery, enlargement of the spleen, &c., are very rare.—*Indian Medical Gazette*, Calcutta, November 1, 1871.

On the Therapeutical Properties of the Eucalyptus globulus.—The 'Gazette Hebdomadaire' calls attention to the properties of the *Eucalyptus globulus*, which have been lately further examined in reference to the treatment of disease. It has been ascertained that the medicinal efficacy of this tree is due to the presence of an essence, or rather a kind of liquid camphor, for which the name of *eucalyptol* has been proposed. Applied externally, this essence is slightly stimulating, and administered internally, in large doses, it is a stimulant. It is eliminated by the kidneys and the lungs, and M. Gimbert has observed augmentation in the proportion of urea in the urine; but the effects of the Eucalyptus are due, probably, not only to the essence, but also to the other matters contained in the leaves, as tannin, bitter principles, and, perhaps, some other substance not yet isolated. The effects of the Eucalyptus in the treatment of marsh fevers are said by several physicians to be most remarkably beneficial; but M. Gubler, who has examined the therapeutical properties of the plant, has not yet observed its action in a sufficient number of cases to pronounce a decided opinion on its febrifuge virtues; but in the diseases of the respiratory organs its effects seem to be very satisfactory, especially in sub-acute or chronic bronchitis, with muco-purulent secretion. M. Gubler, among all the preparations of the Eucalyptus, prefers the powder, which he administers in the dose of four to sixteen grammes (a gramme is about fifteen grains) in the twenty-four hours. A decoction of the leaves, which ought to be made rapidly, furnishes a very active preparation, which may be used externally or internally. The *eucalyptol*, or essence, may also be

easily employed in capsules; and other modes of application have been proposed, such as cigars made of the leaves, fumigations with the decoction or the essence, or the medication of the atmosphere breathed by the patients.—*Gazette Hebdomadaire*, February 9th, 1872.

On the use of Bromide of Potassium in the Bite of the Rattlesnake (Crotalus horridus). By Dr. E. A. Anderson, Wilmington, N.C.—Poisoning by the bites of venomous serpents is of frequent occurrence in the Southern States of America, and especially in the region where Dr. Anderson resides, and he determined to try the efficacy of bromide of potassium in very large doses in such cases. He gives the details of two cases in which the treatment was successfully employed, but it should be mentioned that large doses of alcohol were also administered. The first case occurred in 1867, the patient being a young lady, who was bitten in the hand while picking some violets. The pain was excruciating, and the local and general symptoms were of the most dangerous character. Whiskey was given in large doses every fifteen minutes, and fifteen grains of bromide of potassium every hour. At the end of four hours relief was obtained, as was shown by the returning warmth of the surface of the body, the diminution of pain, and sleep. The patient eventually recovered, although the bitten part sloughed away. Dr. Anderson remarks that many cases of rattlesnake-bite recover under the exclusive use of alcohol, but many die in spite of its employment. He attributes the successful result of the case just recorded in great measure to the bromide, which allayed pain, produced sleep, and seemed to cut short the dangerous symptoms, and he is satisfied that death would have ensued if stimulants only had been employed. The other case was that of a young negro, and occurred in 1870. The man was bitten by an enormous rattlesnake in the knee. The symptoms were similar to those recorded in the first case, and were so severe that the patient appeared moribund. Brandy was forced down his throat, however, at regular and short intervals, and as soon as he was able to swallow, twenty grains of bromide of potassium were given every hour until he had taken 240 grains. Recovery was gradual but complete.—*American Journal of Medical Sciences*, April, 1872.

German Experience of the Use of Chloral Hydrate. By various Authors.—Dr. Alois Monti, of Vienna, has lately communicated to the 'Jahrbuch für Kinderheilkunde' some observations on the use of chloral hydrate in the diseases of children. He found that when doses of the drug suitable to their age were administered, the children exhibited a slight redness of the face, and a condition similar to intoxication, passing off into deep sleep. The pulse was at first somewhat accelerated and afterwards retarded, the respiration was unchanged, and the temperature of the body was reduced. Only very large doses produced a well-marked numbness of the skin and consecutive anæsthesia, with contraction of the pupils. The sleep was normal, and on waking there remained neither sleepiness nor congestion of the head, nor any other disturbance of sensation. The hydrate sometimes produced vomiting, but never diarrhœa, and the

urine after twenty-four hours contained sugar. The dose was from $\frac{6}{100}$ to $\frac{35}{100}$ of a gramme (a gramme is about fifteen grains) for newly born children, and proportionally larger doses as the infants grow older. As to the special uses of the hydrate in children's diseases, Dr. Monti's remarks are as follows:—1. The hydrate fulfils the indication of causing sleep as well and completely in children as in adults. 2. In convulsions, whether symptomatic or idiopathic, the hydrate acts on the symptoms, and is only contra-indicated when the convulsions are complicated with inflammation of the bronchi, the lungs, or the heart. It must be used with great caution in the case of weak and anæmic children. Spasm of the larynx has been relieved by the hydrate, and the drug deserves notice in the treatment of chorea. In hooping-cough it seems to be useful in the first stage, but injurious in the third by diminishing expectoration. On the other hand, the chloral hydrate is uncertain and useless as an anæsthetic for surgical purposes, or in the treatment of any form of tetanus.

Dr. Kühn, of Haina, has communicated to the 'Berlin Klinische Wochenschrift' his experience of the combination of chloral hydrate with morphia in the treatment of chronic cases of excitement in insanity, and in acute mental diseases caused by alcohol. The large doses of chloral hydrate necessary in delirium tremens make the employment of this drug rather dangerous. But Dr. Kühn gave to some cases of paralytics seized with occasional fits of excitement, and to patients suffering from dipsomania, a series of doses of chloral hydrate, with hydrochlorate of morphia, without any unfavorable consequences.

Dr. Stark has also published in the 'Würtemberg Correspondenz Blatt' some remarks on the use of the hydrate in the treatment of mental disease. He recommends the combination of chloral hydrate with morphia (called by him morphio-chloral) in all cases where it is desired to produce a rapid relief of mental excitement. Dr. Stark considers that the hydrate belongs to the class of the most powerful sedatives and narcotics; that it is indicated in convulsions, nervous excitement, and agrypnia, and in the relief of pain; that it should be employed with caution when there are complications with affections of the stomach, lungs, or intestines, and in cachectic persons; that the dose of one or two grammes is generally sufficient; that morphio-chloral is generally preferable to chloral hydrate; and that subcutaneous injections of chloral, or the use of chloral hydrate in combination with injections of morphia, are to be rejected from practice.

Dr. Josef Steinhaus has published in the 'Vienna Medical Press' some observations on the use of chloral hydrate in surgical operations, but it is alleged by other writers that such employment of the drug is so dangerous as to forbid its use.—*Schmidt's Jahrbücher der Gesammten Medicin*, April, 1872.

On the Use of Hydrate of Chloral in Puerperal Convulsions.—By Dr. J. G. Swayne, of Bristol.—Dr. Swayne relates two cases of puerperal convulsions, in which the administration of the hydrate of

chloral appears to have been beneficial. In both instances there was albuminuria, but both patients eventually recovered. In the first case there were twins, one child being delivered by the forceps; in the second case the patient gave birth to a dead child. In both instances bleeding was adopted. The hydrate of chloral was injected into the rectum in both, in the dose of thirty grains dissolved in water, and in the second case the drug was also given internally, as the convulsions continued. In the latter instance thirty grains of chloral were prescribed for a dose, and by a mistake of the nurse the dose was repeated in two hours instead of four, so that a drachm was given in the space of two hours, but no unfavorable result ensued except prostration. Dr. Swayne, in commenting on the cases, remarks that the hydrate of chloral performs only a subsidiary part in the treatment, for he has the highest opinion of the efficacy of bleeding in puerperal convulsions, and he even thinks that this measure removes the albuminuria, which is probably at the root of the affection. From what he has seen of the action of chloral he believes that its operation in puerperal convulsions resembles that of chloroform, but that it is more steady and persistent in its effects, and is much more manageable, because the dose may be regulated with greater nicety. The dose required to produce a decided effect must be large, at least forty grains at a time, and this may be repeated in three or four hours.—*British Medical Journal*, December 30th, 1871.

On the Internal Use of Carbolic Acid. By Drs. Neumann, Hertel, Güntz, Gärtner, and Rothe.—Dr. Neumann, of Vienna, has made several experiments on the effects of carbolic acid in the lower animals and in man. In the former he found that in cases of poisoning by this substance there was congestion of the brain-substance and fatty or granular degeneration of the liver. In man he gave the acid internally in different cases of psoriasis, and found that it produced some effect upon this disease, inasmuch as it reduced the hyperæmia, but it produced no effect on the thickening of the cutaneous tissue; it was, therefore, more successful in the acute than in the chronic cases. Dr. Neumann, among other conclusions which he draws from his experiments, thinks that the internal employment of carbolic acid is useful in scaly skin diseases, although its administration will be limited, owing to its unpleasant and sometimes dangerous effects, but that it may be recommended for external use in parasitic skin diseases, and as a stimulant application in chronic inflammations.

Dr. Hertel, of Copenhagen, admits that the internal use of carbolic acid requires some care, but thinks that it is not dangerous. He gave it to forty hospital patients, and found that nervous symptoms occurred only in one. The acid acts especially on two symptoms which are of frequent occurrence in skin diseases, namely, itching and hyperæmia of the layers of skin which are involved. The first symptom is sometimes suddenly allayed, and the diminution of hyperæmia is especially shown in cases of psoriasis and eczema. He

gave it to adults in the form of pill, and to children in an emulsion with sugar, mucilage of acacia, and yolk of egg. He found the acid of great service in prurigo, especially in allaying the itching of that disease. He also used it with some benefit in acute lichen and in urticaria, but it was useless in acne.

Dr. Güntz, of Dresden, employed pills of carbolic acid in some cases of prurigo and cutaneous pruritus with some benefit. No bad consequences or symptoms of poisoning supervened in any case, even in children.

Dr. Gärtner, of Würtemburg, employed carbolic acid internally with extraordinary success in gangrenous bronchitis, and Professor Leyden, in Königsberg, also employed it externally in four cases in gangrene of the lung with good effect, inhalation being first practised.

Dr. Rothe, of Altenburg, recommends the internal use of carbolic acid in diarrhœa and cholera nostras; he has employed it frequently in the cases of children, and almost constantly with the best results. Out of fifty cases of diarrhœa only two were fatal when treated with the acid, and of these one was complicated with marasmus and the other with convulsions. In diarrhœa and vomiting in older children and in adults the use of carbolic acid in increasing doses according to the age proved a safe remedy. Three cases of cholera nostras were treated with equal success, the patients being between fifty and sixty years old. The symptoms were severe; there was constant vomiting, with copious stools, sunken features, cold clammy skin, painful cramps in the calves of the legs, and a small, thready, frequent pulse. In all three cases the purging and vomiting ceased after the first doses, and the cure was accomplished in two or three days. Dr. Rothe has not yet had an opportunity of testing the carbolic acid in epidemic cholera, but in the event of an epidemic of that kind occurring he would employ it, and the more so as he has never observed any appearance of poisoning from its internal use. The decided benefit which he saw produced in cholera nostras would induce him to recommend it also in epidemic cholera.—*Schmidt's Jahrbücher der Gesammten Medicin*, April, 1872.

On the Physiological and Therapeutical Properties of the different Immediate Principles of Opium. By Dr. Rabuteau.—In a long and interesting series of researches Dr. Rabuteau has extended the investigations of Claude Bernard upon the physiological and therapeutical properties of the constituents of opium. Dr. Rabuteau has examined these properties, not only as they affect the lower animals, but also the human subject, and he has, moreover, considered the alkaloids of opium in relation to their *anodyne* and *anexosmotic* effects. [By the word *anexosmotic* Dr. Rabuteau indicates the property of preventing the flow of liquid through the intestinal walls into the intestinal canal.—Reporter.] He has specially examined these two effects, because opium is very frequently employed to allay pain and to arrest diarrhœa; and he has not only examined the properties of the alkaloids of opium, but also those of the other constituents, such as meconic acid, meconin, &c. It is already well known that the

activity of the immediate principles of opium is not the same in each; but Dr. Rabuteau shows, besides, a fact which was not previously known, namely, that the order of activity of these same principles is not the same in man and in the lower animals. He also demonstrates, by the evidence afforded by his own experiments as well as by those of his predecessors, the principles of opium which cause sleep in the human subject, those which allay pain, those which arrest diarrhœa, and, lastly, those which act upon the system in a more energetic and dangerous manner when given in large doses. The principles which have been discovered in opium, and the order in which they are described by Dr. Rabuteau, are as follows:—*Basic principles.*—Thebain, papaverin, narcotin, codein, narcein, morphin, opianin, porphyroxin, pseudo-morphin. *Other principles.*—Meconic acid, meconin, water, caoutchouc, resin, fatty matter, gum, mucilage, and extractive matters.

The following are among the more important results of Dr. Rabuteau's researches on these substances:—*Thebain* produces, in the lower animals, violent convulsions, similar to those caused by strychnia, but in man it is far less poisonous than the latter alkaloid. In order to ascertain the *anexosmotic* properties of this and other principles, Dr. Rabuteau drew out from an aperture made in the abdominal walls of certain animals a knuckle of intestine, into which, after tying it, a solution of sulphate of soda was introduced, and then the intestine, tied at both ends, was returned into the abdomen, the animal having previously had a solution of thebain injected under the skin. In the case of this alkaloid the portion of intestine was examined after the death of the animal, and was found to contain a large quantity of fluid. Thebain, therefore, does not counteract the effect of purgatives, or, in other words, is not anexosmotic, and, consequently, is not an opiate preparation which produces constipation or arrests diarrhœa. Dr. Rabuteau thus summarises the properties of thebain:—It produces convulsions, and is poisonous in the lower animals, but is less active than strychnia; it is not very poisonous in man; it does not prevent the exosmotic currents of the intestine; it is not soporific, but it increases the anæsthetic effect of chloroform; and it is anodyne.

As it is impossible, from want of space, to specify the researches made by Dr. Rabuteau on each constituent of opium, we can only give his results, which are chiefly as follows:—*Papaverin* is much less active than thebain, and produces hardly any effect when administered in moderate doses to the lower animals, but in large doses it produces convulsions in frogs. In the case of the human subject it possesses but little activity in rather large doses, as, for instance, twenty centigrammes ($\frac{20}{100}$ of a gramme, about fifteen grains being about equivalent to a gramme); but in larger doses it is poisonous, and causes convulsions; it does not arrest diarrhœa, or, in other words, it is not anexosmotic; it is not soporific, but it assists the anæsthetic action of chloroform. *Narcotine.*—With regard to this principle, Dr. Rabuteau confirms the results of previous observers as to its negative characters. *Codein* is more poisonous to man than thebain, but, on the

other hand, according to the experiments of Claude Bernard and Dr. Rabuteau, thebain is the more poisonous to the lower animals. Codein is dangerous to man in large doses, it is very slightly soporific, very slightly anodyne, and is not anexosmotic, and, therefore, is useless in the practice of medicine. *Narcein*, according to Claude Bernard, is the most soporific of the bases of opium, and is less poisonous than thebain, codein, and papaverin; but Dr. Rabuteau, from his more recent researches, thinks that although narcein is more soporific in dogs than morphin, yet that in the human subject morphin is superior in this respect. Narcein augments the action of chloroform, and it is anodyne and anexosmotic. *Morphin* is the most poisonous and the most soporific of the principles of opium in man, but it is also the most anexosmotic, as has been proved by experiments similar to those related in connection with the properties of thebain. It is also anodyne, as is well known, but it presents this inconvenience, that it deranges the system by causing loss of appetite, nausea, and vomiting. The other principles of opium are of so little importance in medical practice that it is unnecessary to refer to them in detail.

Dr. Rabuteau concludes his paper with some observations on the simultaneous action of chloroform and the alkaloids of opium. It has been found that the lower animals were much less sensible to pain when they were subjected to the influence of chloroform and also the opiate preparations; thus, in cases where both agents were administered, the insensibility to pain remained, even when the chloroform was no longer administered, and yet the animals did not sleep. The alkaloids of opium, therefore, generally continue the anodyne action of chloroform, although they are not all soporific, but they almost all possess the property of diminishing sensibility. Claude Bernard and Nussbaum have found that when an opiate subcutaneous injection was performed in certain cases of operation on the human subject, and chloroform was subsequently given, the patient did not wake as usual, but continued to sleep, and during this sleep there was insensibility to punctures, incisions, and even the actual cautery. Dr. Rabuteau, therefore, thinks that insensibility might be obtained with greater certainty and safety by the combined administration of a solution of chloroform and an opiate than by giving either agent alone.—*Gazette Hebdomadaire*, April and May, 1872.

On Bromal Hydrate. By Drs. Berti and Namias.—Drs. Berti and Namias communicate the following results of the experiments made by them upon the use of bromal hydrate:—1. Even in small doses and suitably diluted this substance produces burning in the throat, pyrosis, vomiting, and diarrhœa. 2. It is best taken in an emulsion, but by long use, even in this combination, it produces vomiting and purging. 3. Even relatively large doses do not cause sleep, and do not tranquillise the patient. In epilepsy, bromal hydrate is useless, and only in one case, where bromide of potassium was abandoned, the hydrate cut short the attack. 4. Subcutaneous injections cause phlegmon, and produce besides no subsequent effects

on the nervous system. 5. Rubbed up with an equal quantity of fat, bromal hydrate causes erythema when applied to the skin, but it differs in its injurious effects from sinapism in this respect, that this erythema is accompanied with deep-seated and very obstinate infiltration of the subcutaneous areolar tissue. 6. Even as a stimulant bromal hydrate possesses no superiority over similar agents, but is inferior to them in this respect, that it acts more slowly and its operation is less limited. 7. Poisonous doses produce death with sopor, paralysis of motion and sensation, and rapid or gradual cessation of the respiratory movements. Bromal is by no means to be employed as an internal or subcutaneous medicine, since it is more caustic than other agents. It is only when it is desirable to bring a large quantity of fluid to a particular part of the skin, as in cases when dry cupping-glasses are used, that bromal hydrate may be tried externally.—*Journal de Bruxelles*, April, 1871.

On the Therapeutical Action of the Calabar Bean. By Dr. Sée, of the Hôpital de la Charité, Paris.—The Calabar bean is principally employed on account of its action on the iris, the effect produced being the contraction of the pupil. Dr. Sée, in explaining its physiological properties, refers to the mechanism of the iris, which obeys two sets of muscles, of which the one, composed of circular fibres and acting as a true sphincter, is supplied by filaments of the third pair of nerves, and the other, formed of radiated fibres perpendicular to the first-named fibres, is under the control of the great sympathetic system of nerves. The pupil, therefore, may be contracted by two opposite causes, namely, either by the *tetanisation* of the sphincter or by the paralysis of the radiated fibres which antagonise the circular ones. According to Dr. Sée, the Calabar bean tetanises the muscle of the iris, just as atropine paralyses it, by acting on the terminal extremity of the third pair of nerves. This view, however, is opposed to the fact that the Calabar bean paralyses the spinal cord, but Dr. Sée admits both facts, although they appear contradictory to one another. In consequence of its paralysing effects on the spinal cord, Dr. Sée has employed the bean in tetanus, and he states that a former pupil of his, having tried the bean in eighteen cases of this disease, more than half of which were traumatic, succeeded in curing thirteen. Dr. Sée also relates the case of a patient, treated by him, in whom ophisthotonos and trismus were relieved by the Calabar bean, and the cure was complete. The Calabar bean must be considered, in the first place, according to Dr. Sée, as an excitor of the third pair of nerves, and as afterwards acting on the muscles of the intestines, and then on the vaso-motor nerves, which, under the influence of the drug, increase the pressure of the vessels by contraction, soon followed by relaxation. This consecutive effect explains the increase of the tears, the saliva, and the perspiration, and the occurrence of conjunctivitis all which have been observed to follow the use of the bean. The Calabar bean retards the pulsation of the heart, by exciting the terminal extremities of the vagus nerve, the trunk of that nerve remaining excitable. Belladonna, on the other hand, poisons by paralysing

the terminal extremities of that nerve.—*L'Union Médicale*, April, 1872.

On the Use of Baths in Acute Rheumatism, attended with Head Symptoms and High Temperature. By Dr. HENRY THOMPSON, of the Middlesex Hospital.—A case has lately occurred in the Middlesex Hospital illustrating the beneficial effects of the bath in acute rheumatism, attended with head-symptoms and high temperature. Dr. Thompson, in his remarks on the case, observes that it is only now and then, in a few rare and scattered instances, that acute rheumatism proves fatal by an unexpected outbreak of overpowering nerve-symptoms, and such a result would have ensued in the instance recorded, according to Dr. Thompson's opinion, if the bath had not been used. The fatal issue in similar cases was formerly referred to the occurrence of metastasis, meningitis, and the like, but now it is said to be due to hyperpyrexia. To this last term, if used in a practical sense, Dr. Thompson makes no objection, as the importance of very high temperature cannot be overrated as a symptom; but in a pathological point of view he thinks that its influence has been exaggerated, for the nerve-symptoms invariably precede the hyperpyrexia. Nevertheless a high body-heat, ranging from 108·6 degrees to 112, is incompatible with life, and it is necessary to lower it by such means as are available, and Dr. Thompson thinks that the use of the bath is the best therapeutical agent. The temperature of the bath is 90 to 95 degrees in the first instance, and is gradually reduced by the addition of cold water to 70 degrees. The clinical thermometer must be practically the best guide for the employment of the bath, for at a lower body-temperature than 102·5 degrees it would not be desirable to use it. It is important to observe that, in the case recorded by Dr. Thompson, the most severe and extensive chest complications, such as pneumonia, pleurisy, bronchitis, and pericarditis, underwent no perceptible change for the worse in consequence of the bath. The case itself is recorded at length, with the thermometrical observations taken at frequent intervals from day to day, and the effects produced by the baths, which were eight in number, are accurately noted. The case, however, although terminating in recovery, was a tedious one, and the convalescence was exceedingly slow.—*British Medical Journal*, August 3rd, 1872.

On the Electrolytical Treatment of Scirrhus. By Dr. J. J. CALDWELL, of New York.—Dr. Caldwell gives the history of two cases in which electro-chemical action was employed for the relief of cancer. In the first there was a hard, large, and painful tumour occupying the base of the mammary gland and involving the nipple; the surrounding tissues were hard and irregular, and the skin was tense and of a purple colour, with two or three ulcerating points. The treatment was commenced on the 25th of September, 1871, by the application of a constant current of electricity of from eight to ten cells, and lasting from ten to fifteen minutes. This treatment

was continued until the 30th of October, when the tumour had nearly disappeared. From the general appearance of the tumour, and the examination by the microscope of a small portion of it, the growth was considered to be of a cancerous nature, but it is right to mention that one of the medical gentlemen who was asked to examine a specimen which was sent him did not admit it to be so. Dr. Caldwell attributes the disappearance of the tumour, in this and other cases, to the decomposing power of the galvanic current, by which suppuration is set up and the growth is destroyed, and he thinks that the solvent action of the electric current is analogous to that exercised by the vital forces on the food in the stomach. In illustration of this action he rendered a piece of beef fluid by placing it in a solution of chloride of sodium at a temperature of 100° to 110°, and acting upon it for thirty-six hours by electrolysis. In the second instance recorded by Dr. Caldwell the result was unsuccessful. This latter case was undoubtedly one of cancer in an advanced stage, and of the form known as fungus hæmatodes. The electrolytic treatment was tried only three times.—*New York Medical Journal*, May, 1872.

On the Therapeutical Uses of the Continuous Electric Current. By Mr. CALLENDER, of St. Bartholomew's Hospital.—Mr. Callender has tried the effect of the continuous electric current in the treatment of various cases, more especially with reference to its electrolytic action on tumours of different kinds. In order to ascertain the effect of the current upon an acute abscess, he employed it in the case of a powerful labouring man, a needle connected with the negative pole of the battery being passed into the abscess while the positive pole was applied to the adjacent skin. The current was applied twice, but its use had no effect on the progress of the abscess, which took its usual course and broke. Mr. Callender then relates three cases of tumour in which the current was applied, and in one the tumour seemed to melt away and disappear under the treatment. In the remaining two cases the results were not so successful, one of them being an instance of recurrent fibroid tumour and the other case being one of cancer of the breast. The successful case was one of recurrent fibroid tumour in the right loin, and four operations had been practised for its removal. Before the wound from the last operation had closed, one or two isolated nodules, growing up from the granulating surface, made their appearance, and it was to these that the electric current was applied. The patient was fifty-one years of age, and his general health was very good. Two needles were used, one from the positive pole being inserted into the growth, the other, connected with the negative pole, being applied to its surface. Some immediate sloughing was caused about the track of the first needle, but eventually the wound healed soundly. Mr. Callender, in drawing some conclusions from his cases, divides the action of the continuous current into that of electrolysis, and that of cauterisation. With reference to the first, he thinks that in no instance has the progress of the tumour been visibly influenced by the use of the

current in the sense of its causing wasting or shrinking of the growth independently of the cauterising action, but he intends to give that plan of treatment a further trial. With regard to the cauterising action, however, he thinks there is no question as to the efficacy and value of the remedy. With twelve cells and two needles small growths may be rapidly destroyed without pain on the part of the patient, and in the successful case recorded they were destroyed so effectually that the skin healed over the parts of the granulating surface from which they were growing. For the cauterising of such recurrent masses, especially for that of the small nodules which spring up about the scar-tissue after removal of a cancer-growth, and which, if allowed to increase, necessitate for their removal a considerable operation, Mr. Callender thinks that the continuous current seems to offer a handy and efficient remedy.—*British Medical Journal*, Feb. 10th, 1872.

On the Therapeutical Use of Guarana (Paullinia sorbilis). By Dr. S. WILKS, of Guy's Hospital, and Dr. P. W. LATHAM, of Cambridge. —Dr. Wilks draws the attention of the profession to guarana as a remedy for sick headache. Some two years ago he received a letter from a medical gentleman in British Columbia, inclosing two powders, which were recommended with much confidence as a remedy for headache. Dr. Wilks tried the powders first on himself, but without much effect, and did not therefore indorse his friend's recommendation. Lately, however, he received a letter from Dr. Wood, of Montreal, also recommending guarana as a remedy for headache, and explaining that he himself had obtained relief from the use of the drug. Dr. Wilks now determined to try the remedy in a systematic manner, and he procured a packet of the powders from Mr. Hooper, the chemist, and prescribed them for various patients with very encouraging results. The guarana has for some time been a favourite medicine in France for sick headache, as well as for various forms of neuralgia, given in doses of ten to fifteen grains or more of the powder once or oftener in the day. The plant from which it is obtained is the *Paullinia sorbilis*, a native of Brazil, and belonging to the natural order Sapindaceæ. The seeds, roasted, bruised, and pressed into cylindrical masses, constitute the guarana paste, which, when finely pounded, is known as Paullinia powder. It contains, in addition to empyreumatic oil (developed in the process of roasting) and tannic acid, *guaranin*, a substance identical in composition with caffein. It is therefore efficacious as an astringent, and has much the same effect on the nervous system as tea or coffee. Paullinia powder is used, not only in headache and neuralgia, but also as an astringent in catarrhal diarrhœa, and in blenorrhœa of the urinary organs.—*British Medical Journal*, April 20th and 27th, 1872.

REPORT ON PHYSIOLOGY.

By Henry Power, F.R.C.S., M.B. Lond.,

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1. Sir W. Gull, Bart., and H. Sutton. *On the Pathology of Chronic Bright's disease with Contracted Kidney.* ('Proceedings of the Roy. Med. Chir. Soc.,' vol. vii., No. 2.)
2. Dr. Duval. *Recherches Experimentales sur les rapports d'origine entre les Globules de Pus et les Globules blancs du Sang dans l'Inflammation.* ('Brown-Séquard's Archives de Physiologie,' Nos. 2—3, 1872.)
3. D. Paraskeva and J. A. Zallonis. *De l'Inoculabilité de la Tuberculose.* ('Gazette Médicale de Paris,' No. 17, 1872.
4. Dr. Thomas R. Fraser, M.D. *On the Antagonism between the Actions of Physostigma and Atropia.* ('The Transactions of the Royal Society of Edinburgh,' vol. xxvi.
5. Benj. W. Richardson, M.D. *Report on the Physiological Action of Organic Chemical Compounds.* ('Report Brit. Assoc.,' 1871.)

Although the first five subjects do not fall within the range of physiology they are of so much present interest that we shall offer no excuse for introducing them as histological memoranda at the beginning of this report.

1. Sir William Gull and Dr. Sutton consider that the visible morbid changes in the kidneys are due to the formation of a fibroid or hyaline-fibroid substance in the intertubular parts, including the vessels, and to atrophy of the tubular and intertubular structures of the kidney. This formation commences in different parts of the kidney, commonly near the surface, and it also seems to commence in the outer coats of the arterioles and in the walls of the capillary vessels. Its subsequent contraction draws the Malpighian bodies together, compresses the urinary tubules and vessels, and ultimately obliterates them. The hyaline degeneration, they maintain, may commence in other organs than the kidneys; and, on the other hand, the kidneys may undergo extreme degenerative changes without being attended with the cardiac hypertrophy, and the other vascular changes characteristic of the condition known as chronic Bright's disease. In the discussion which followed the reading of the paper, Dr. Broadbent adhered to the view of Dr. George Johnson, namely, that the thickening of the vessels was due to hypertrophy of the muscular coat, and supported his statement by an account of the effects of the inhalation of nitrate of amyle on the tension of the vessels, as indicated by sphygmographic tracings. Dr. Bastian and Dr. Dickinson, on the other hand, gave clinical evidence in favour of the author's views. Dr. George Johnson thought that the hyaline appearance described by Sir W. Gull and Dr. Sutton was in reality only the swollen and translucent state of the tunica adven-

titia, so commonly produced by the imbibition of fluid, especially when that fluid is mixed with glycerine or acetic acid.

2. Dr. Duval's experiments were made upon the cornea, and upon the mesentery of frogs, mice, and rats, with a view of determining the origin of pus globules. The cornea was irritated by being touched with a point of nitrate of silver, or by having a silk thread passed through it. The results of careful microscopic investigation showed that the resulting inflammation of the cornea did not proceed from the periphery towards the centre, but radiated from the seat of lesion. In the parts undergoing change no free white globules were ever seen; the corpuscles always proceeded from a cellular proliferation. The point de départ of the metamorphoses was always the plasmatic cellule (corneal corpuscle), which, far from remaining unaltered, becomes hypertrophied and produces new globules. In the experiments on the mesentery the animals were subjected to the action of curara, and the membrane examined with high powers. It was then seen that *no escape* of the white corpuscles from the vessels occurred, but that as the circulation became retarded accumulation of these corpuscles occurred at the angles of division and along the sides of the vessels, and that coincidently hypertrophy and multiplication of the corpuscular elements, naturally existing external to the vessels, took place. In a note appended to the paper M. Vulpian, one of the editors of 'Brown-Séquard's Archives,' expresses himself as being opposed to M. Duval's statements, and as agreeing with Cohnheim and Waller in attributing the origin of pus to the escape of white corpuscles of the blood from the smaller vessels.

3. Paraskeva and Zallonis from their experiments on animals and man draw the conclusion that tubercle *is* capable of being inoculated.

4. We can here only briefly call attention to the long and valuable essay by Dr. Fraser, on the 'Antagonism between the Actions of Physostigma and Atropia,' which he shows is so remarkable and decided that the fatal effect, even of three and a half times the minimum lethal dose of physostigma, may be prevented by atropia. He gives the details of three series of experiments by which it has been ascertained what is the maximum dose of physostigma that can be counteracted successfully by atropia;—what is the dose of atropia that can counteract any given dose of physostigma;—and what relationship exists between the doses with which this mutual counteraction occurs, and the length of the interval of time by which the administration of atropia precedes or follows that of physostigma.

5. Dr. Richardson gives the result of his inquiries made with a view of determining what is a dangerous, and what a fatal, dose of chloral hydrate. He thinks 140 grains would be dangerous, and that even 80 grains might be fatal. When too large a dose has been taken the patient should be kept in a warm air (90°—100° F.), and should be supplied with half a pint of warm milk, mingled with a little lime water, every two hours. Artificial respiration, in desperate cases, must be maintained. Dr. Richardson also describes the effects of anhydrous chloral, metachloral, bromal hydrate, and nitrate of amyl,

(entering into details in regard to the action of the latter agent upon the lungs), nitrate of ethyl, nitrate of amyl, sulpho-urea, chlor-ethylidene, and hydramyl. He further describes the action of chloroform, methylene, absolute ether, and hydramyl, on the minute circulation of the frog's web. He states that he was never able to observe any change in the physical characters of either the red or white blood-corpuscles. The first sign of arrested circulation commenced in every case on the venous side of the circulation, and consisted of a sort of pulsation, or to-and-fro movement of the current through the vein, the venous current then soon became slower and the vein dilated, while the arterial current often remained long unchanged. In every case the minute circulation remained long in force after the respiration had entirely ceased, but the cessation of the minute circulation was the sign of irrevocable death. When the circulation was at its lowest it was still maintained by to-and-fro, with occasional steady, onward, movements in the arteries. In the veins too there were now and then, short movements, first, as of impulse towards the heart, and then of retreat backwards; these movements in the veins were succeeded invariably by an increased and more perfect action of the arteries; during this state the capillaries were left empty and transparent. Certain peculiarities characterised the action of each vapour. In a section on the convulsive movements that occur during narcotism, Dr. Richardson divides them into primary, or those beginning in the lesser or pulmonary circulation; and, secondary, or those beginning in the larger or systemic circulation, and attributes both to the disturbance of the balance of supply of blood to the nervous and muscular centres.

J. W. S. Arnold. *Hæmatoxylin as a Staining Material for Animal Tissues.* ('Philadelphia Medical Times,' July 1, 1872.)

Dr. Arnold calls attention to the fact that the special action of *carmine* is upon the *nucleus* of the cell, whilst the protoplasm is left almost entirely unchanged, and further that carmine does not stain well preparations that have been artificially hardened by chromic acid or bicarbonate of potash. In logwood he has found a colouring agent that he thinks is in these respects superior to carmine. (It has already been used by Rollett and others.) To obtain the colouring solution of logwood he finely pulverises the ordinary extract in a mortar, and adds about three times its bulk of alum. The two ingredients are well triturated for twenty minutes and mixed with a small quantity of distilled water. The solution, when more water has been added and filtration effected, should present a clear, somewhat dark, violet colour. If a dirty red is obtained more alum must be incorporated, and the mixture again filtered. To one ounce of this fluid two drachms of 75 per cent. of alcohol must be added. Tissues stain very rapidly when placed in this solution, a few minutes being all that is required; if a slower tinting be required the fluid may be diluted with a mixture of one part alcohol and three parts water. Whether the tissue has been previously hardened in alcohol, or in any of the chromic compounds, it is coloured equally well—the nucleus of a most brilliant purple, the cell-body of a distinct neutral tint.

J. J. Woodward. *On an improved Method of Photographing Histological Preparations by Sunlight.* Washington, 1871.

Gibbons Hunt, Wm. Walmsley, D. S. Holman. *Discussion on the Preparation and Preservation of Tissues.* ('Philadelphia Med. Times,' May 1, 1872.

Cells—Connective tissue—Tendon—Bone.

1. Ludwig Letzerich. *Contribution to the Physiology of Ciliated Cells.* (Translated by Dr. Kloman in 'Philadelphia Medical Times,' Dec. 15, 1871.)
2. Jos. G. Richardson. *On the Structure of the White Blood-Corpuscles.* ('American Journal.')
3. Dr. Geltowsky. *On the Action of Quinine on the Colourless Blood-Corpuscles.* ('Practitioner,' June, 1872.)
4. C. Binz. *Quinine and the Colourless Blood-Corpuscles.* (In the 'Practitioner,' No. li, Sept., 1872.)
5. Königschmied. *Verbreitung der becherformigen Organe der Zunge.* (In 'Centralblatt für die Med, Wiss.,' No. 26, 1872.)
6. Renaut. *Note sur le Tissu Muqueux du Cordon Ombilical.* ('*Gélatine de Wharton.*') (Brown-Séquard's Archives, 1872.)
7. Karl Aeby. *Ueber vergleichende Untersuchungen der Knochen.* (In 'Centralblatt, f.d. Med. Wiss.,' No. 7, 1872.)
8. L. Ranvier. *Des Elémens cellulaires des Tendons, &c.* (In Brown-Séquard's 'Archives de Physiologie,' 1869, p. 471.)
9. Boll. *Untersuchungen über den Bau und die Entwickelung der Gewebe.* (In Max Schultze's 'Archiv,' Band. vii, p. 275.)
10. Mitchell Bruce. *On the Structure of Tendon.* (In the 'Quarterly Journal of Microscopical Science,' April, 1872.)

1. Dr. J. Richardson concludes from his experiments that the white blood-corpuscle is a cell composed of a nucleus (or nuclei) which latter possesses the power of voluntary amœboid movement, is insoluble in water, but is capable of slowly imbibing that fluid and increasing to nearly double its normal size. The cell-wall of the corpuscle is a membranous envelope, insoluble in water, too thin to exhibit a double contour with a magnifying power of 1200 diameters, but firm enough to restrict the movement of the contained granule. Its exterior is somewhat adhesive so that surfaces or particles coming in contact with it are liable to become attached thereto. Some phenomena observed lend countenance to a theory that this membrane is dotted with minute pores, which permit delicate threads of the soft protoplasm to be extruded, and whose edges, if the projection still continues during the amœboid movement, are carried outwards as a sheath to all except the extreme point of the narrow tongue-like process. The material occupying the space between the capsule and the nucleus, denominated the protoplasm of the cell (the fibro-plastin of Professor Heynsius), is a soft jelly-like matter in which the power of amœboid motion resides. It appears to be soluble in water, and saline solutions in all proportions, and when freely diluted loses its amœboid power, which, however, is regained in a majority of cases when the excess of fluid is withdrawn.

The laws by which leucocytes take up and part with liquids seem to be simply those of the dialysis of fluids through animal membranes by endosmosis and exosmosis, as studied by Graham on a larger scale in 1855.

2. Dr. Geltowsky shows that quinine arrests the movement of the colourless globules of the newt's blood when used in the proportion of one part to 800 or 900 parts. The globules of the blood of the female resist longer than those of the male. The globules of the blood of animals enfeebled by the loss of blood in previous experiments resist the action of quinine for a shorter time. He thinks the action of quinine on the blood-corpuscles can only be obtained on the stage of a microscope, since on injecting into the blood doses of quinine which caused the death of the animal, no such action is observable on the colourless blood-corpuscles. Therefore, in whatever way quinine acts in diminishing the quantity of white blood-corpuscles in the blood of leucæmia it is not by the direct action of the quinine on the white blood-corpuscle.

3. Binz, who first noticed the action of quinine on the white corpuscles, thinks that a sufficient quantity of quinine *can* be taken not to kill, but materially to diminish the number and energy of the white corpuscles.

4. Königschmied describes as existing in the guinea-pig a structure analogous to the papilla foliata of the tongue of the rabbit, pig, squirrel, rat, and man. It consists of three parallel grooves on the sides of which are bud-like bodies, exactly similar to the gustatory chalices of Lovén and Schwabe and arranged in from three to four rows.

5. Dr. Karl Aeby gives the results of his investigations upon the structure and composition of the femur and tibia of the ox at different ages. He finds that they contain on the average 4 per cent. more salts than the corresponding bones of man, a higher specific gravity, and a smaller proportion of water. With the advance of age the bones of the ox become more calcareous and heavier. In the third year there is a remarkable diminution of the sp. gr. indicating a process of reabsorption coincident with the period of development.

6. Ranvier's investigations were made on the tendon of the tail of young rats which he removed from the body, stretched on a slide kept in position by a drop of sealing-wax and rendered transparent with dilute acetic acid. He regards tendon as composed of cylindrical fasciculi of connective tissue, of very variable diameter, and bounded by a special layer or a kind of membrane formed by annular and spiral fibres. These fibres appear to be a simple thickening of the membrane, since like this, and unlike elastic tissue, they are coloured by carmine. Between the fasciculi are two kinds of cells. One is globular, granular, amœboid, and in all respects like the white blood-cell, the other forms quadrilateral nucleated plates, the edges of which are bent round to form a tube, and these tubes run parallel to and between the tendinous fasciculi.

7. Boll describes the cellular elements of the embryonal tendon of

the diaphragm as granular, elastic, quadrilateral plates, arranged in a row behind each other and bent to include the tendon bundles. Each plate is furnished with a median "elastic stripe" occupying the whole longitudinal axis of the cell running parallel to the long axis of the bundles. The stripe is coloured more deeply than the rest of the cell, and being placed end to end, the appearance is presented of rows of stained stripes, straight or wavy in accordance with the state of tension or relaxation of the tendon.

8. Mitchell Bruce agrees with Boll in regard to the form of the cells, which he considers to be united into a continuous layer by intermediate cementing substance. The fibril bundles are so enveloped in this sheet that each is surrounded to the extent of half its circumference by one of the series of cells forming the sheet, and the whole tendon if unravelled would present the appearance of a sheet of paper upon which and beneath which, alternately, a series of rods had been placed. Each rod (respectively a tendinous fasciculus) would then be half covered by the sheet which would represent the continuous plane of the compound cells, and if a succession of such layers were superimposed on one another each fasciculus would receive a complete investment.

Muscles.

1. G. M. Humphry, M.D., F.R.S. *Lectures on Human Myology delivered at the College of Surgeons.* ('British Med. Journal,' June, 1872.)
2. W. Marcet, M.D., F.R.S. *On the Nutrition of Muscular and Pulmonary Tissue in Health and Disease.* ('British Med. Journal,' Feb. 10th, 24th, and Aug. 3rd, 1872.)
3. J. H. Floegel. *On the Striated Muscles of Mites.* ('M. Schultze's Archiv für Mik. Anatomie,' Band viii, p. 69.)
4. F. Merkel. *Der Quergestreifte Muskel.* (Idem, p. 244.)
5. W. Dönitz. *Beiträge zur Kenntniss der Quergestreiten Muskelfasern.* ('Reichert und Dubois Reymond's Archiv,' 1871, p. 434.)
6. G. R. Wagener. *Neben der Querstreifen der Muskeln.* ('Sitzungsberichte der Gesell. der Gesammten Naturwiss. zu Marburg,' 1872, p. 25.)
7. B. Danilewsky. *Zur Chemie des Tetanus.* ('Centralblatt für die Med. Wiss.,' No. 28.)
8. L. Oser and W. Schlesinger. *Experimentelle Untersuchungen über Uterus-bewegungen.* ('Stricker's Wien. Med. Jahrbucher,' 1872, Band ii, p. 30.)

1. Professor Humphry's lectures, and his still more recently published 'Treatise on Myology,' contain the results of many careful dissections of the muscles of the *Cryptobranchus japonicus*, the *Uromastix spinipes*, *Lepidosiren annectens*, *Dog fish*, *Ceratodus*, and *Pseudopus*, with an able exposition of the general relations and arrangement of the muscles in vertebrate animals.

2. Dr. Marcet maintains that animal tissues are formed by three

classes of constituents. The first has been called the tissue proper; it is insoluble in water. The second has been called the nutritive material, it is soluble and consists of a mass of colloid substances, the composition of this nutritive material being the same as that of the tissue proper. The third has been called effete material, and consists of crystalloid substances which, it is concluded, are on their way out by diffusion. He gives directions as to the mode in which each of these may be separated from each other.

3. J. H. Floegel describes the striated muscular tissue of Trombidium, a species of mite resembling the red spider found in gardens. He macerated the whole animal for an hour or two in a one per cent. solution of perosmic acid, washed it in water, and dissected it in a dilute solution of glycerine. The anisotropous doubly refracting transverse discs are already found to be deeply stained, whilst the isotropous singly refracting substance is scarcely allied. He describes the sarcolemma as sending in septa dividing the fibril into compartments, the middle of each of which is filled with the doubly refracting, while the ends are occupied by the singly refracting, substance. The transverse discs consist of separate columns which are imbedded in a medium that is scarcely coloured with perosmic acid. In many substances the transverse discs break up with two valves which are separated from one another less deeply coloured disc (? Hensen's intermediate disc). The two singly refracting terminal zones of each compartment contain very constantly a layer of very small granules. The muscular fibres of this Trombidium must, therefore, be considered to be composed of a fluid or very watery matrix which is scarcely coloured with perosmic acid and is filled with denser columns; the fibrils, and each fibril, has a granule connected with it at a certain point, the mutual apposition of these forming a transverse band across the muscle. From septum to septum the contents of each compartment are as follows:—1. A singly and slightly refracted substance slightly coloured with perosmic acid. 2. A granule which stains deeply with perosmic acid, and with its neighbours forms a transverse layer of granules. 3. The same as 1. 4. The double and strongly refracting substance becoming deeply stained excepting sometimes near its centre. 5. As 3. 6. As 2. 7. As 1, to which may be added 8, the deeply tinted transverse septum.

4. Merkel's views as regards the structure of muscle agree in many points with those of Floegel. He recommends for observation the thoracic muscles of the fly or bee, and states that the phenomena of their contraction may be distinctly seen when the fibres are immersed in albumen. The appearances presented are, first, the terminal discs approximate, lateral expansion of the fibrils then occurs quite gradually, and this is accompanied by a narrowing of the part occupied by the contractile substance. When the contraction is completed, the terminal discs are seen to be closely approximated. The part occupied by the contractile substance, which, when the muscle is at rest is only dimly visible, becomes marked by a well defined outline, and to superficial observation the decrease in length appears to be compensated for by the increase in breadth. This, however, is by no means the case,

but the dimly defined spot representing the position of the contracted substance, has become quite disproportionately attenuated, whilst the terminal discs are not, as might be expected, attenuated, but are actually thickened. It would hence appear that the muscular fibre has undergone a change in its histological character, and is not merely a shorter and thicker body than when at rest. The change consists in this, that the contractile substance which in the resting fibre is accumulated around the median disc of each muscle-element, leaves this position during contraction, and becomes applied to the corresponding terminal discs. Hence, instead of each element containing, as it does during rest, an entire transverse stria in its middle, it exhibits during contraction one half of this stria at each end. Merkel recommended the chelæ of a crab to be immersed in alcohol. The alcohol penetrates but slowly, and so, although it causes the outer fibrils to contract, the innermost ones are dead, and therefore relaxed before the fluid reaches them, and sections show all the intermediate stages.

5. Dönitz maintains the old view of fibrils as opposed to the compartment theory of the structure of muscle.

6. Wagener admits the existence of Hensens's intermediate band, but maintains there are from two to eight adjoining striæ, which though very firm are constantly present. He describes the contraction of the muscle of an insect in the following terms:—The anisotropal substance with Hensens's intermediate stria, and the adjoining striæ shorter and approximate to one another, and then the two isotropal striæ, which at first are separated by the broad anisotropal substance, become so closely compressed together that they are only divided by a faint line.

7. Danilewsky shows that the quantity of albuminous compounds diminishes, though to an inconsiderable extent, in tetanus. The alcoholic extract of tetanized muscle contains more nitrogen than that of resting muscle, which, from various considerations, render it evident that during contraction there is an increased disintegration of the muscle substance. During contraction a phosphorized body—lecithin—is produced. Warm alcohol withdraws from muscle exclusively the products of its retrogressive metamorphosis.

8. Oser and Schlesinger state as the essential result of their experiments, that movements of the uterus may be induced by arrest of the respiration, by rapid hæmorrhage, and by arrest of the passage of the blood to the brain. They found that the best animals for the purposes of experiment were kittens under the influence of woorara.

1. Louis Ranvier. *Recherches sur l'Histologie et la Physiologie des Nerfs.* ('Brown-Séquard's Archives de Physiologie,' No. 2, March, 1870.)
2. Jobert. *Contribution à l'Etude du Système Nerveux Sensitif.* ('Robin's Journal de l'Anatomie,' Band vii, 1870—71, p. 611.
3. Fr. Goltz. *Ueber den Einfluss der Nerven-Centren auf die Aufsaugung.* ('Pflüger's Archiv,' v, p. 53.)

4. Stieda. *Bau der Nervösen Central-organe.* ('Centralblatt für die Med. Wissenschaften,' No. 13—14, 1872.)
5. Ph. Lussana. *Sur les Nerfs du Gout.* ('Brown-Séquard's Archives de Physiologie,' 1872, p. 150.)
6. J. Gottstein. *Ueber den feineren Bau und die Entwickelung der Gehörschnecke der Saügethiere und des Menschen.* ('Max Schultze's Archiv,' Band viii, p. 145.)
7. Nuel. *Beiträge zur Kenntniss der Saügethierschnecke.* (Idem, p. 200.)

1. Ranvier proposes in his memoir to describe a new feature in the structure of the nerves, and to investigate what are the means by which the nutrition and disintegration of nerves are accomplished. In regard to the first point he calls attention to the existence of certain annular constrictions in nerve-fibres which have hitherto escaped the observation of histologists, and which occur at tolerably regular intervals of one twenty-fifth of an inch. He states that he has studied the nerves after being subjected to the influence of the picrocarminate of ammonia, osmic acid, and nitrate of silver, and he gives the details of the mode of preparation in each instance. We shall mention only the mode with picrocarminate of ammonia. He adds a solution of carmine in ammonia to picric acid to saturation, then evaporates to one fifth of its bulk; the cold liquid throws down a precipitate containing but little carmine. The bitter mother-liquor, evaporated to dryness, gives the solid picrocarminate of ammonia in the form of a crystalline powder of the colour of red ochre. This powder dissolves readily in water, and a 1-100th is a convenient strength. The nerve is placed in a few drops of the solution as soon as excised and teazed out with needles, but not to too great an extent. The examination may be made with a power of from 300 to 600 diameters when the nerves appear as if living, neither the membrane of Schwann, nor the nuclei, nor the cylinder axis, being visible as distinct structures, but at the intervals mentioned above there are distinct annular constrictions. After the lapse of a minute or two the nerve presents modifications above and below the strangulations. The myeline loses its transparency, and the cylinder axis becomes yellow. It presents a double contour. The external contour is straight, the internal is less marked and sinuous. After some time the cylinder axis becomes reddish at the level of each constriction, but the constriction itself remains uncoloured. He thinks the constricting ring itself is formed by a thickening of the membrane of Schwann. The nerve tubes destitute of the white substance of Schwann, or the fibres of Remak, do not present the constrictions. Nuclei, strongly tinted, are rendered visible in the substance of the membrane of Schwann, and these he thinks belongs to this membrane. When treated with osmic acid the nerves with low powers appear like black hairs, divided at regular intervals with clear constrictions, which constrictions, examined with high powers, appear as biconcave menisci on each of the faces of which the myeline or white substance of Schwann terminates by a convex extremity, whilst the cylinder axis is continued through them. Each segment of the nerve-fibre, thus formed, has a single nucleus situated at nearly equal distances

from the constrictions. Treated with nitrate of silver the ring is tinted black, and the cylinder axis, above and below it, for a short distance presents a curiously laminated structure.

2. Jobert describes the termination of the tactile nerves in the apices of the digits of the Racoon. He found in the papillæ similar tactile corpuscles to those found in the apes and in man. In the subcutaneous connective tissue he found Vater's corpuscles and intermediate forms of terminal corpuscles. In the second part of his paper he states he has been able to substantiate the presence of a medullary sheath in the nerves of various Mollusca.

3. Goltz poisoned two frogs by woorara to eliminate the effect of voluntary muscle contractions, and as soon as paralysis had made its appearance the brain and spinal cord were destroyed in one animal, the other being left intact; the heart was then exposed in both, and after they had been suspended vertically the aorta was divided just above the bulbus. It was found that a free discharge of blood took place from the frog that still possessed its spinal cord and brain, whilst from the other only a few drops escaped. The explanation of this difference is no doubt to be found in the circumstance that in the decerebralized frog the vessels have lost their tone and cease to force blood towards the heart. The dorsal lymph-sac of each animal was now filled through a cut in the skin of the head with a measured quantity (10—25 cm.) of a one per cent. solution of common salt. The following remarkable phenomena were then observed: — From the animal, still in possession of its brain and spinal cord, a fluid, at first bloody but subsequently clear, flowed continuously, drop by drop, from a nick made in the skin of the belly. The fluid had been propelled onwards from the lymph-sac, and had passed through the head. On the other hand, from the animal from which the brain and spinal cord had been removed not a single drop escaped. In the former animal the tension of the lymph-sac had diminished whilst in the latter it still remained full. The skin of the former was covered with abundant secretion, was strongly wrinkled owing to contraction of its involuntary muscular fibres, and preserved its natural fresh colour. The skin of the latter was dry, flaccid, and discoloured.

From these observations M. Goltz draws the conclusion that a force proceeds from the brain and spinal cord which, even after total suppression of the blood circulation, causes a free current of blood to pass from the lymph-sacs into the blood-vessel. Further experiments showed that the spinal cord was the principal agent, as the removal of the brain occasioned but little difference. The transmission of induction shocks through the lower limbs of the frog that still retained its brain and spinal cord increased the amount of fluid discharged threefold, whilst it had no effect on the other. On the other hand, if in the former the tone of the vessels was lowered by smart blows on the belly, much less fluid was discharged. It is thus clear that the nervous system does exert an influence upon the process of absorption. It is not easy, however, to explain the nature of that influence. Is it of an indirect nature referable to the tone of the vessels, and if so, how

does this influence the process? or does the nervous system, as Goltz believes, act more directly upon the epithelial cells of the lymphatics, which like glandular epithelium are active agents, and in some unexplained manner urge the fluid absorbed onwards.

M. Lussana states that in consequence of the criticism of MM. Vulpian, Schiff, Vizioli, and others, he has been induced to repeat the experiments made by himself and Inzani, and he adduces various pathological cases in favour of his former views. These views are—1, that the lingual of the fifth presides over the sense of taste in the anterior portion of the tongue; 2, nevertheless, the gustatory fibres are not derived from the fifth but from the chorda tympani branch of the seventh, since in cases where the fifth is paralysed in all its branches, either by section or disease, the sense of taste is often preserved, whilst, on the other hand, if the facial be similarly diseased or injured, the sense of taste in the anterior part of the tongue is wholly lost.

Blood—Circulation.

MM. Mathieu and V. Urbain. *Des Gaz du Sang. Expériences physiologiques sur les Circonstances qui en fait varier la proportion dans le Système Artériel.* (In Brown-Séquard's 'Archiv de Physiologie,' 1872, p. 190.)

MM. Mathieu and Urbain contribute the results of their inquiries into the proportion of gas contained in the blood of different arteries. The researches of Claude-Bernard have shown that the quantity of gas contained in venous blood varies with the condition of activity or of repose of the organ whence it proceeds. Though the greater or less amount of oxygen absorbed by the tissues explains these variations very well, a second interpretation has been proposed, namely, that the diminution of oxygen in the blood is due to intro-vascular oxidation taking place at the expense of the blood itself. On this view the oxidation which is well marked in the veins commences in the pulmonary capillaries, and continues in the arteries, causing a gradual disappearance of the oxygen, the loss attaining its maximum in the veins. As a consequence of this, the arterial blood does not everywhere present the same composition. MM. Estor and St.-Pierre actually concluded from their experiments that whilst the blood of the carotid contained 21·06 per cent. of oxygen, that of the crural artery contained only 7·62 per cent., so that 13·44 per cent. of oxygen disappears in the course of a second or two; the authors attribute this sudden change to the occurrence of incessant intra-vascular oxidation. MM. Mathieu and Urbain's experiments may be divided into two groups: first, those in which the proportion of gas contained in the blood of arteries of nearly equal diameter was compared; and, secondly, those in which that from arteries of different calibre were examined. As regards the former, they found that no remarkable difference could be detected between the carotid and crural artery: in one case, for example, the proportions were for the carotid, oxygen 20·45 per cent., nitrogen 1·6° per cent., carbonic acid 48·18 per cent.; and for the crural, oxygen 18·08 per cent., nitrogen 1·60 per cent., carbonic acid 44·23 per cent.,

the slight difference being due to the effect of the first bleeding. On comparing the blood of large with that from small arteries, the amount of oxygen in the former was greater than in the latter, the difference sometimes amounting to 3 per cent.

A. Schmidt. ('Centralblatt für die Med. Wiess.,' No. 16, 1872.)

A. Schmidt finds that, in blood drawn from the living vessels, a ferment originates from the action of the blood-corpuscles which causes the coagulation of fibrin. The material for the fermentative process is afforded in the fibrinogenous and the fibrinoplastic substances, which must be simultaneously exposed to the action of the ferment in order that the fibrinous coagulum may be formed. The action of the blood-corpuscles upon the fermentation process is dependent on the hæmoglobin, though other substances that condense oxygen upon their surfaces and catalytically decompose peroxide of hydrogen act in the same way, such as spongy platinum, carbon, &c. The transudates found in the cavities of the dead body for the most part contain the two fibrin-generators, but no ferment, and they will coagulate on the addition of the ferment. In some the fibrinoplastic substance is absent, and requires to be added before coagulation will occur. Neutral alkaline salts and low temperatures arrest the process. To obtain the ferment, the albuminous substances of the blood must be coagulated by maceration for fourteen days in concentrated alcohol; the fluid is then filtered, and the residue on the filter dried at a low temperature, pulverised, and extracted with glycerine.

Mr. H. L. Smith. *An Improved Mode of Observing Capillary Circulation.* (In the 'Monthly Microscopic. Journal,' 1872, vol. i, p. 268.)

Mr. H. L. Smith states that if we grasp a frog in the hand and plunge it into water about as warm as can be conveniently borne (? 120° Fahr.), we shall find that in a few moments the frog will become perfectly rigid. It may now be removed and laid upon a plate for dissection. The heart may be displayed and its action beautifully seen, and if now the animal be placed in warm water, the lungs will immediately float out, and by a suitably contrived stage the circulation may be examined. It is better, however, to draw out gently the large intestine by means of blunt forceps, and then to spread the mesentery on the glass of the frog-plate. The circulation can be easily examined with a one-fourth or one-fifth, or even a higher power 'immersion lens.' The animal will remain perfectly quiet for a long time.

REPORT ON PATHOLOGY AND PRINCIPLES AND PRACTICE OF MEDICINE.

By Francis C. Webb, M.D., F.L.S.,
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Pathology of the Blood in Chronic Diseases.—In January of the present year Dr. Adolph Lestorfer announced the discovery of certain corpuscles in the blood of patients the subjects of syphilis. The corpuscles were not to be seen in fresh blood, but in blood which had been kept from twenty-four hours to four days. The corpuscles were discovered by the use of high magnifying powers, Hartnach's eye-piece No. 3 and immersion lens No. 10. The corpuscles were described as minute, bright bodies, some immovable, some in a state of undulation. Some of the bodies exhibited projections. As they were watched from day to day they enlarged in bulk and numbers. The projection appeared to be a kind of sprout; some of the bodies had one projection, some three or four. The projections were sessile or had a minute pedicle. The corpuscles were irregular in shape. After eight or ten days a vacuola was formed in the larger corpuscles, which extended over the whole body and terminated its further development. Dr. Lestorfer stated that he had examined in a similar manner the blood of patients suffering from gonorrhœa, diphtheria, eczema, typhus, elephantiasis, and lupus, but never had found anything to be compared with the appearances he found in syphilitic blood. He gave to the bodies the name of "syphilis-corpuscles." The discussion which has arisen out of Lestorfer's announcement has led Professor Stricker to re-examine the whole subject. Professor Stricker commenced his observations by examining the blood of three syphilitic men; in all three, in the course of a few days, the Lestorfer's corpuscles were found. He found that the temperature at which the blood was kept had a great influence on the production of the corpuscles. In a chamber heated to 22 Cent. he watched the process of the growth and development of these bodies. He writes:—"On the following day I took again a fresh specimen of blood from a syphilitic patient, had my chamber warmed, placed a portion of a specimen under Hartnach's immersion lens No. 10, and watched. About fifteen minutes had passed since the drop of blood was taken, when it seemed to me that I could detect, in the otherwise clear plasma-interspace, a few granules. In the course of ten minutes more these granules had assumed so decided a character that I could clearly define their positions. At the same time it appeared to me as if numerous fresh granules were making their appearance in the same interspace. At the end of half an hour the granules which had first appeared were already so large as to enable me to recognise them as the bodies I was looking for. At the end of an hour and a half the first granules had grown to the size of a nucleus of a pus-

corpuscle, and the whole plasma-interspace was scattered over with similar but smaller granules. There was no longer the slightest doubt; I had before me Lestorfer's bodies; they had made their appearance and grown up in a clear plasma-interspace under my own eyes." Stricker finds that whilst it is true that the bodies bear sprouts, some of the sprouts arise, not through a process of growth, but through the apposition of other bodies. He watched the process by which a smaller corpuscle which had been lying in the neighbourhood of a larger one approached the latter, and became attached to it like a button. With regard to Lestorfer's statement that the bodies sometimes grow into sack-shaped bodies or tubes, he states that he only once saw a corpuscle with a long adherent tube, but he saw the tube originate by the arrangement in a row of three small corpuscles. The interspaces and constrictions between them became effaced, and a homogeneous tube projecting from the large corpuscle was completed. The chemical reaction of these bodies was different immediately after their first appearance from what it was when they had been developed to a large size. When fully developed, they are but little affected by alkalies and acids; they shrink a little, but are not destroyed. In an early stage they are destroyed by acids and alkalies, and even by water. Stricker observed the phenomenon of repeated approach and separation between larger and smaller bodies, a phenomenon which he says can only be explained upon the supposition that their bond of union is contractile, and that the bodies themselves are organisms. In answer to the question whether these corpuscles are only to be found in the blood of syphilitic persons, he gives a decided negative. In 13 cases of general syphilis he found the bodies in large number in 9 cases; in 2 they were not to be found; in 2 they were so few that he considered the result negative. He found no trace of the corpuscles in the blood of 10 healthy, well-nourished persons, in 1 case of pneumonia, 1 of heart disease, 4 of typhus abdominalis, 1 of typhus exanthematicus, and in 10 cases of smallpox. They were found in 2 out of 10 cases of lupus. Stricker, however, was led to inquire whether the occurrence of the corpuscles might not be connected with deficient nutrition. He therefore observed the blood in several chronic affections. He found the corpuscles in large numbers in 1 case of carcinoma ventriculi, in 2 cases of tuberculosis, in a case of Bright's disease with disease of the heart, and in a case of anæmia following variola. He concludes that these new elements of the blood are seldom present in sound persons and in persons with acute diseases, but are frequently present in persons suffering from long-continued disturbance of nutrition, and in persons affected with syphilis.

Lestorfer also has published some observations showing that sarcinæ, or what closely resemble sarcinæ ventriculi, are found in the blood of healthy men after it has been kept to the third and fourth days.—*Archiv. für Dermatologie und Syphilis*, IV Jahrgang, erstes Heft, 1872; *Medicinische Jahrbücher*, 1872; *Amer. Journ. Med. Sciences*, July, 1872.

On the Injection of Pus into the Medullary Canal of Bones.—M. Demarquay has laid before the French Academy of Medicine the account of some experiments in which he injected pus into the medullary canal with the view of determining whether it is absorbed and carried into the circulation. Pus taken from a human subject—pure and putrid—was injected into the medullary canal of the femur of rabbits. Twelve experiments were recorded. The following were the results in each case. All the animals died after presenting during life the following phenomena:—(1) Alteration of the hair, (2) sensible loss of flesh, (3) elevation of temperature from 38 to 39°; it has been noted as high as 41 or 42°, diminishing a little before death. The autopsies showed deep and superficial phlegmon more or less marked on the side operated on. 2. Pulmonary congestions; pneumonic patches more or less marked; once pulmonary gangrene. 3. Congestion and softening of the liver, spleen and kidneys. 4. Metastatic abscesses of the liver in various degrees of development. M. Demarquay is convinced of the importance which is to be attributed to osteomyelitis in the theory of septicæmia or purulent infection. The same lesions were produced in these experiments as follow the injection of putrid matters into the jugular or crural veins.—*Gaz. Hebd. de Méd. et de Chir.*, Oct., 1871.

On the Local Action of Pus and Putrid Substances.—According to some experiments by S. Samuel there probably exists a specific toxical agent in putrid matters. Putrid substances injected under the skin of a rabbit's ear, in a state of concentration, and in a quantity of twelve or fifteen drops, produce in the living animal a putrid process, which, developing more or less rapidly, is characterised by swelling, greenish coloration of the tissues, and an intense putrid odour. This physiological reaction is also observed with substances which do not present the putrid odour nor the characteristic chemical reactions. Symptoms of septicæmia, due to the absorption of the injected matters, follow the local phenomena. The chemical agents whose presence has been established in putrid matters do not give rise to this reaction. This is the case with leucine, butyric acid, valerianic acid, carbonate of ammonia, sulphurous water, sulphocarbonic acid. Sulphide of ammonium, injected under the skin, produces patches of gangrenous inflammation more resembling those produced by putrid poison, but they differ in not giving off the putrid odour. The physiological reaction of putrid pus is not altered by the quantities of butyric, formic, or valerianic acids it contains.—*Medic. Central. Zeit.*, and *Gaz. Hebd. de Méd. et de Chir.*, Oct., 1871.

Hectic Fever.—Dr. Francis D. Condie lays down the proposition that hectic fever, however distinctly developed, is by no means pathognomonic of pulmonary tuberculosis, even when accompanied by impeded or disturbed respiration, cough and expectoration. The symptoms of hectic mark, usually, cases of spurious consumption (chronic bronchitis and pneumonia), while hectic fever is the attendant upon a large number of cases of disease in which no affec-

tion of the respiratory organs is present, as is proved by post-mortem examinations. Again, it is not invariably the attendant on pulmonary tubercular phthisis. Louis found it wanting in one case out of ten. According to the author's experience, however, the fever is absent less frequently than this. Neither is it true that hectic is dependent on slow continued suppuration in some internal part or organ, or that it is caused by the reabsorption of pus from internal or deep-seated abscesses. It is found in tubercular phthisis even before tubercular deposit has taken place or any abscess has been formed in the lung. In no case has it direct relation to the extent of the suppuration going on at the time, or which has preceded it. In patients of an eminently strumous diathesis it is one of the first indications of mischief going on in the lungs. It is of common occurrence in cases of debility when the patient has been subjected for a long time to some source of irritation, although it is certain there is no suppuration going on in any part of his organism. This is especially the case in patients of a strongly marked nervo-lymphatic temperament. On the other hand, it is absent in very many cases of long-continued suppuration when there is an impediment to the escape of the pus, as in cases of abscess under deep fasciæ. Hence the presence or absence of hectic affords no certain diagnosis as to the presence of pulmonary disease or between tubercular and spurious phthisis.—*Amer. Journ. Med. Sciences*, April, 1872.

Dengue.—Surgeon Sparrow, of the 89th Regiment, gives an account of the dengue fever as observed in Cannanore amongst officers and troops who had just arrived from Aden, where dengue was prevailing. Isolation proved successful in preventing the extension of the disease. A general uniformity of symptoms characterised all the cases. There was a sudden invasion of the disease by pain of limbs, followed by the hot stage of fever, intense frontal headache, furred tongue, acute pain of joints (the most marked symptom during the progress of the disease), the pain so severe in some cases that the patient was seen to fall to the ground as though paralysed; the stage of pyrexia lasted three days, when, with the exception of continued pain in the joints, all the symptoms were relieved; there was clean tongue, normal pulse, cool and moist skin. On the fifth day a burning pain of the palms of the hands was complained of, on examination they were found to be swollen and covered with red-coloured blotches. The eruption then extended up the arms, and in most cases the entire body became covered with the erythematous eruption, resembling that of scarlatina; fever returned and lasted twenty-four hours; after which there was gradual abatement of the symptoms, but soreness of the feet and swelling of the hands, with inability to close them, continued. In many the pains in the joints continued at intervals for a month. No delirium attended any of the cases. Except in one or two cases where there was slight diarrhœa, there were no sequelæ; relapses, consisting of increase of articular pain, with slight return of fever, were observed in three or four.—*Madras Monthly Journal of Med. Science*, May, 1872.

Scorbutus.—Dr. A. Porter gives an account of scurvy as observed in fifty cases which occurred during five years in Akola jail; 78 per cent. of the cases occurred during the months of July, August, and September, when fresh vegetables are most scarce; owing to the rains. The coldness and moisture of the season seemed to have little to do with the causation of the disease; the majority of the patients were not exposed. The attacks could not be attributed to deficiency of salt in the diet. In 92 per cent. of the cases weakness and listlessness were present on admission; in 4 cases only was pain in the limbs and back the first symptom noted by the patient; pain in the limbs and back were absent in 14 of the 50 cases, but in 7 of these there was pain in the limbs. Nyctalopia was present in 74 per cent. of the cases, and in 60 per cent. was the first symptom to make its appearance. In one case there was a burning sensation in the conjunctiva. Petechiæ and ecchymoses were noted in 24 per cent., but this symptom was not easily recognised on account of the darkness of the skin of the patients. In 88 per cent. the gums were livid and swollen; in 6 cases they were pale and contracted; in 10 cases soreness of the gums, with difficulty of mastication, was the first symptom observed. Bloatedness of the face was absent in 26 per cent. of the cases. Fetid odour of the breath was present in every case except two. Respiration was generally normal; in only 10 cases was it noted above 20 in a minute, in 5 of these there was chest complication. Hard swellings in the flexures of the joints were present in only 60 per cent. The pulse in uncomplicated cases was weak and generally under 80; morning temperature 98°—99°, evening temperature a degree higher. The appetite was bad in 14 cases; the tongue furred in 30 cases; livid in one, swollen in none. The bowel swere costive in 4 cases, loose in 3, dysenteric in 1, normal in the remainder. Sleeplessness was noted in 58 per cent. One patient died from syncope. Effusion into the pleura was present in 2 cases, and pulmonary congestion in 3. In 2 cases there was lupoid ulceration near the outer malleolus. The urine was pale of low specific gravity, and contained indican. It was otherwise healthy. The treatment consisted of full diet with fresh vegetables, arrack and lime juice. The gums were sponged with a solution of nitrate of silver. The author records the post-mortem appearances in six fatal cases. In almost every instance there was emaciation, with effusion into some part of the body. Effusion of blood on the pleura, peritoneum, and beneath the mucous membrane of the stomach and intestines, was observed in some of the cases.—*Madras Monthly Journal of Med. Science,* April, 1872.

Suppuration of one half Lobe of Cerebrum; Consciousness and Ability to Labour Intact; Sudden Death.—Dr. Schwartzenthal relates the case of a day-labourer, æt. 30, who in May, 1871, suffered for two weeks with pain in the head, langour and want of appetite, followed by a severe fever, which lasted a month. He then became apparently convalescent, and resumed his occupation. About four weeks after he left the hospital he received a blow on the head and died

instantly. The posterior half of the right lobe of the cerebrum was reduced to a circumscribed accumulation of pus, while the anterior half of the lobe and the entire left cerebral hemisphere were of a doughy consistence. The cerebellum was to some extent softened. The patient had been pursuing his avocation without apparent difficulty.—*Amer. Journ. Med. Sci.*, from the *Wien. Med. Presse*, 1871.

Cerebro-cardiac Neuropathy.—M. Krishaber has called attention under this name to a class of cases of which he has collected many examples, and which follow an invariable type. They are characterised by four groups of symptoms:—1. Sensorial disturbances. False or perverted conceptions, almost approaching a state analogous to alcoholic intoxication, but without real delirium, the patient always retaining the power of correcting his illusions by reason. A symptom not less constant than aberration is hyperæsthesia. 2. Disturbances of locomotion, consisting chiefly of the loss of the sensation of equilibrium, caused by vertigo and giddiness. Paraplegia sometimes supervenes; sometimes there is more or less marked paresis, affecting nearly all the muscles of the body and evidenced by sensations of lassitude and weariness; sometimes there are involuntary impulses, the patient walking in spite of himself in certain directions; at other times the gait is hesitating and uncertain; in other cases there is excitation in place of paralysis. These different disturbances often succeed each other in the same patient. 3. Disturbances of the circulation. There is great irritability of the vascular system, so that merely moving will send up the pulse 20, 30, or 40 beats. There are also violent palpitations, spontaneous or provoked by a slight cause; irregular action of the heart, small radial pulse, often slow, soft, and very yielding. Sometimes at the onset there are phenomena simulating fever—a rigor followed by a full throbbing pulse, but little increased in frequency. The temperature during the access may go up half a degree, but rarely a degree. These invasions, which seem almost periodical in type, cease spontaneously and only occur in the early stage of the disease. 4. There are secondary symptoms, which are purely individual and consequently variable. There is always excessive nervous irritability. There is a grave and slighter form of the disease; they are distinguished by the character of the sleep which the patient obtains. In the grave form there are insomnia, nightmare, palpitation, and cerebral excitement without delirium. In the slighter form the patients sleeps almost normally.—*Gaz. Hebd. de Méd. et Ch.*, May, 1872.

On the Connection of Sympathetic Insanity with Alterations of the Semilunar Ganglion.—M. Conyba, in a course of lectures on mental and nervous disorders, draws attention to that form of insanity—*la folie sympathique*—which depends upon disturbance in a distant organ having no direct connection with the cerebrum, for instance a disease of the stomach or intestine or some other abdominal viscus. The connection between such cause and the mental alienation is proved by the re-establishment of the normal functions of the cerebrum when the visceral disease is removed. After citing cases in

point from various authors he inquires. What is the mode of production of this species of insanity? Is it produced by the direct action of the diseased organs upon the brain or by reflex action? The impression is transmitted by the sympathetic filaments to the solar plexus and its ganglia, which are reflex centres, whence it is reflected on the brain. The author then recounts two autopsies of patients who had died suffering from sympathetic madness of abdominal origin, which appear to show that the abdominal ganglia of the great sympathetic are concerned in these cases of alienation. To the naked eye there was nothing abnormal in the appearance of the ganglia, but the microscope showed a great number of embryoplastic nuclei, besides a more advanced condition, that is to say, fusiform bodies, and young lamellar tissue running in various directions. The number of nerve-cells was diminished, those which remained were either healthy or atrophied, or filled with pigmentary or fatty granulations. Some presented nuclei, in others they had disappeared. The envelope of some was much thickened. One case was that of a confirmed hypochondriac with delusions, the other was one of suicidal insanity. In the latter case the epiploon was covered with white patches, there were adhesions between portions of the intestine and the liver and spleen, the large intestine was narrowed in places in consequence of the adhesions, and in parts dilated and thinned. The transverse colon was displaced, and had a vertical direction. Lobstein has observed sympathetic madness following alterations of the great sympathetic after diseases of the intestine. Pinel considered hypochandriasis as always connected with lesion of the abdominal sympathetic. Bichat found the semilunar ganglion fibrous, and the author mentions the case of a female hypochondriac in whom the same organ was found fibrous.—*L' Union Médicale*, May, 1872.

The Physical Cause of Prolonged Expiration, and its relation to the Pretubercular Stage of Consumption.—Dr. Samuel G. Amos contends that prolonged expiration in the early stage of phthisis is not necessarily the result of deposit of tubercle or caused by bronchial congestion, but is the result of the want of pulmonary elasticity. It depends upon an error of nutrition, which may be general or local. If general, the contractile power of the muscular and elastic fibres participates in the universal failure of nutrition; the prolonged expiratory murmur is simply one of the manifestations of weak vital power, just as an atonic muscle, a certain quality of an elastic artery, or the non-contractile state of a feeble capillary, is an indication of constitutional debility. Thus, morbidly prolonged expiration may be observed in anæmia, in exhaustive discharges, in advanced stages of typhoid fever, in Bright's disease, &c. In all such cases vital contractility—including the natural resiliency of lung tissue—is everywhere disturbed by the general failure of nutrition, and this may exist without reference to tubercle. Prolonged expiration, practically considered, is frequently characteristic of the pretubercular stage of consumption, but it may be independent of tubercle altogether. If the prolonged expiration be local—whether from local

anæmia, loss of power from the effused products of inflammation, fatty degeneration of the air-cells, or stiffening of their walls from infiltrated tubercle—we are apt to have, with prolonged expiratory murmur, a variety of local and circumscribed sounds, moist and dry râles, corresponding with the seat and extent of the tuberculous deposit."—*New-York Med. Journ.*, March, 1872.

Sudden Death as a Termination of Aortic Insufficiency.—M. Michel Peter, in the course of a clinical lecture on aortic insufficiency, discusses the question of the frequency of its termination in sudden death, or death by syncope. Gendin, and after him Aran and Mauriac, have especially called attention to this possible termination of aortic insufficiency. Stokes maintains, on the contrary, that disease of the mitral valves offers greater chances of sudden death than analogous affections of the aorta. The author maintains that, notwithstanding Stokes's opinion, the doctrine advanced by Gendin is really true. But he insists upon the intervention of the cardiac plexus as a necessary factor in giving rise to a sudden fatal termination in the case of aortic insufficiency. As a practical result he asserts that it is not enough to have diagnosed aortic insufficiency to be authorised to prognosticate as possible sudden death. There must have been noted besides the signs of aortic insufficiency retro-sternal pain, or, *à fortiori*, attacks of angina pectoris, the pain or angina indicating a grave lesion of the aorta and its reflection on the cardiac plexus. It is only when these signs coexist that we should be warranted in prognosticating a sudden fatal termination.—*L'Union Médicale*, Nov., 1871.

The Diagnosis of Anæmic Murmurs.—Dr. J. H. Hutchinson records three cases of cardiac murmur heard at the pulmonary cartilage, where there were marked anæmia and venous hum, in which the murmurs became more intense on the patients assuming the recumbent position. He thinks this point of importance in the diagnosis of anæmic murmur. Dr. Stokes, of Dublin, when speaking of the murmur occasionally heard during the convalescence from maculated typhus, mentions, as one of its characteristics, its frequent diminution or disappearance in the erect position. He says "this may help us in distinguishing it from an organic murmur, but I am unable to say whether the same is not observed in ordinary cases of anæmia."—*Transac. Col. Phys. Philadelphia, Amer. Journ. Med. Sciences*, April, 1872.

Epidemic Malarial Colic.—Dr. F. J. Galt gives an account of an epidemic malarial colic which prevailed at Iquitos, Peru, in the autumn of 1871. Good health and tropical apathy had combined to make the inhabitants indifferent to drainage. About the 10th of October, 1871, after a season of great heat, dryness, and differences in the day and night temperature, the epidemic described began. The cases occurred in a population of different nationalities—Indian, Mestigos, and Europeans, and most of them occurred within four or five weeks. One case came from the Huallaga river, 200 miles above Iquitos. The symptoms, after certain prodromata, were excessive pain referred to the

epigastrium or umbilicus, and shooting to all parts of the abdomen or backwards to the spine, unaffected by pressure; nausea, and persistent vomiting of bilious or watery matters; pulse generally unaffected. On the second or third day yellowness of the conjunctiva, which lasted through the attack and after convalescence. In some cases the body had "that peculiar bronzed and splotched appearance so marked in bad cases of yellow fever." Constipation, unrelieved by purgatives; urine high coloured, no albumen; and early prostration, were amongst the recorded symptoms. Quinia, conjoined with calomel, was tried and soon relieved pain and procured sleep. The epidemic could have had no possible relation with lead as a cause. The author believes that the epidemic was of malarial origin and essentially of a neuralgic character, the solar plexus apparently being affected. The effects of treatment confirmed his view of its origin.—*Amer. Jour. Med. Sciences*, April, 1872.

Chronic Diffuse Nephritis.—Dr. Francis Delafield, after giving a *résumé* of the descriptions by various authors of the different pathological conditions which are grouped under the name of Bright's disease, says that, from post-mortem examinations made at the Bellevue deadhouse, he finds chronic diseases of the kidney may be conveniently classed under three heads:—1. Chronic congestion of the kidney. 2. Chronic diffuse nephritis. 3. Chronic parenchymatous nephritis. With regard to the second, chronic diffuse nephritis, it seems to him highly erroneous to speak of this disease as the latter stages of an acute inflammatory nephritis. It is from its beginning to its close a chronic affection. The lesions in this affection are—1. Granular and fatty degeneration of the epithelium of the tubes, especially in the cortex. Owing to the different character of the epithelium lining the pyramidal and cortical portions of the tubes, the appearances which are perfectly healthy in the convoluted tubes may indicate advanced disease in the straight tubes. It is only in very marked degrees of fatty and granular degeneration that we can be certain of the abnormal character of isolated epithelial cells. In the great majority of cases we can only determine the condition of the epithelium by comparison. To do this we must make thin sections, embracing the greater part of a pyramid and the cortex belonging to it. This section should then be examined with a magnifying power of not over 100 diameters. We can then see whether, in all the tubes, the epithelium has the same transparency, whether the tubes are filled by it in an equal degree, whether it is swollen or flattened, whether it is in place or detached. 2. Dilatation of the tubes is a common but not a constant change. In the pyramids the tubes may not only be larger, but more tortuous. In extreme cases the dilatations form cysts. 3. Formation of cast material in the tubes. This condition, varying in extent, is nearly always present. 4. Increase of the interstitial tissue. This is a constant lesion, varying in amount. 5. Changes in the blood-vessels. The walls of the arteries may be thickened and their lumen diminished. The Malpighian tufts may be shrunken and atrophied, their vessels impervious.

The veins, especially the capillary plexus in the cortex, may be dilated. The Malpighian tufts, the small arteries, and the veins in the pyramids, may be the seat of waxy degeneration. These lesions may correspond to very different gross appearances in the kidney. The organ may be of normal size, may be enlarged, or atrophied. The third form described by the author—chronic parenchymatous nephritis—is a condition met with in the course of severe and exhausting diseases; the kidneys are of normal size or slightly larger, neither congested nor anæmic; the cortex opaque and yellowish white, the markings of the straight and convoluted tubes are lost, or may be nearly perfect. The pyramids appear normal, or their papillæ are a little whiter than usual; the Malpighian tufts and vessels are unchanged. In the convoluted tubes the epithelium may appear normal, or may be swollen so as nearly to fill the tubes, and appear stiffer or more opaque than usual. In the straight tubes of the cortex the epithelial cells are granular, sometimes broken down and detached so as partly to fill the tubes; in the straight tubes of the pyramids the epithelial cells are granular, detached from the basement membrane, sometimes glued together by hyaline-cast matter. In some of these tubes and in Henle's loops there are hyaline casts. Whether this disease be a parenchymatous inflammation or degeneration is uncertain.—*New York Medical Journal*, March, 1872.

On Bronzed Coloration of the Skin.—Professor Béhier maintains that the connection which Addison observed between a special form of asthenia, alteration of the supra-renal capsules and bronzing of the skin, and in the combination of which he recognised a new disease, is far from being clearly established. He shows that alteration of the capsules and asthenia may coincide without any alteration of the skin, that also bronzed skin may be observed without alteration of the supra-renal capsules, also that disease of the capsules may occur without alteration of the skin. He shows that the most frequently observed lesions in the supra-renal capsules are those commonly considered scrofulous or tuberculous, and with these the bronzed skin may be wanting. He also maintains that the general conditions and symptoms which have been attributed to the so-called Addison's disease are variable and uncertain. He argues that, inasmuch as the normal anatomy and physiology of the supra-renal capsules are at present not well known and doubtful, it is impossible to assess the pathological value of the alterations which these organs undergo and of their symptomatology, especially as their pathological manifestations are inconstant. Whilst, therefore, he allows that the phenomena of bronzed skin and capsular disease are worthy of careful study, he denies that the problem has been solved by Addison. The supra-renal capsules are not the *point de départ* of the altered coloration of the skin. M. Landois thinks that bronzed coloration is a symptom which may be met with in all cachexias, that it is only a result of the local transformation of the epithelium—a particular instance of functional disturbance. The objections to this opinion are that bronzed skin is rare in individuals

affected by the cancerous, tuberculous, and other cachexias. But M. Béhier agrees with Landois that the bronzed malady is not a well-established and defined morbid entity.—*L'Union Médical*, April 18—May 2.

On Alterations of the Muscles of the Skin in Cutaneous Affections.—Dr. J. Neumann finds that the smooth muscular fibres of the skin which are found in different parts of the integument (besides the muscular layers in the scrotum, penis and anterior part of the perinæum) are constantly swollen in variola. This tumefaction differs from the muscular hypertrophy which is produced in red lichen, icthyosis, elephantiasis, prurigo, sclérèma, and has been demonstrated by Derby, Rossbach, and Köbner. This hypertrophy in these diseases is supposed by Derby to depend upon extra contraction of the smooth muscular fibres in attempts to expel the secretions of the skin through follicles strictured at the orifice. Sometimes the smooth fibres are so largely developed as to give the idea of there having been a new formation of smooth fibres in the connective tissue. Atrophy of these organic muscular fibres is characteristic of senile atrophy of the skin.—*Allgem. Wiener Mediz. Zeit.*, Sept., 1871, and *Gaz. Hebd.*, Oct., 1871.

Ainhum.—M. Leroy de Mericourt has called attention to a curious affection which is not uncommon amongst the negroes of Brazil, and to which they give the name "Ainhum;" it does not influence the health of the individual attacked, and its effects do not extend beyond the organ which the disease affects, *i. e.* the fifth toe. It seems to consist of a fatty degeneration of almost all the anatomical elements of the toe, and inevitably leads to its loss. It is more common amongst the negroes of Brazil than the negresses; its causes are unknown. The "ainhum" commences by a slight semicircular depression, occupying the internal and lower surfaces of the root of the toe, coinciding exactly with the digito-plantar furrow; there are no inflammatory phenomena, internal pains, or permanent ulceration. The toe increases gradually in size, the furrow extending towards the dorsal and external surfaces; at last the extremity of the toe becomes doubled or tripled in volume. When the circular furrow is very deep the toe, which is irregularly rounded, acquires great mobility; it interferes with walking, becomes liable to painful shock, and the patient applies to have it removed. M. Cornil has examined the tissues of a toe affected with ainhum. On section there is observed considerable hypertrophy of the papillary layer and of the epidermis covering it. Below it the dermis presents no hypertrophy; the cellulo-adipose tissue acquires considerable development, but it is normal. The condition resembles that of lipomatous tissue. The glands are normal; the osseous tissue itself has become fatty. At the furrow the mucous layer is very thin, the papillæ are considerably atrophied, whilst the anatomical elements themselves do not appear altered.—*L'Union Médicale*, December, 1871.

On Diseases Produced by Animal Poisons.—M. Chauveau divides contagious diseases into three categories. First, into parasitic diseases, properly so-called, due to the presence of animals or vegetables which multiply by direct or alternate generation; diseases which are the result not of the parasites themselves but of the irritation and local mischief they determine. In these maladies if the parasites are very small in number, or of a nature but slightly aggressive, or if they do not attack organs of great importance, their presence is compatible with tolerable health, such, for instance, is the muscular trichina, the hepatic hydatid, &c. Another category of contagious maladies of a parasitic nature comprises the class of septic or septicoid affections, which in the present state of science are to be considered as the products of a rapid multiplication in the blood of proto-organism-ferments, whose decomposing action upon that fluid produces a sort of poisoning, more or less grave, according to the species and the individual conditions of the subjects attacked. These affections may complicate virulent diseases, properly so called, but they are not to be confounded with them. Lastly, there is the category of contagious maladies which comprehends the true virulent diseases. They are distinguished from the preceding by their cause which, does not present the characters of a parasite ferment. When we endeavour to determine the intimate cause of the group, by the study of the physiological evolution of the virulen elements, we find that the virulent activity develops and confines itself strictly to the germinal matter or granular protoplasm of the néoformations. In the fluids to which the specific irritation, due to the presence of the virulent principle, gives rise, the virulent activity is principally found in the free granuliform particles derived from the germinal matter and held in suspension by the liquid. The virulent activity is absolutely absent from the liquid portion of the fluids. Plasms and serums, in which the granuliform elements float, are, if deprived of these elements, quite inactive. The latter alone are the poisonous agents. In respect to the origin and mode of development of these agents we may say that the intimate cause of virulence resides in the specific-properties acquired by protoplasm from elements which arise and develop in contact with a virulent germ, giving rise to the same specific properties when producing similar germs.—*Gaz. Hebdomad. de Med. et Chir.*, Nov., 1871.

CONTRIBUTIONS TO MEDICAL LITERARY HISTORY.

Adversaria Medico-Philologica.

BY W. A. GREENHILL, M.D. OXON.

Part XIII.

(*Continued from p.* 281.)

δρακοντίδες is said by Rufus Ephesius[1] to be a name given by Hippocrates to the veins taking their origin directly from the heart; but the word has not been found in any part of the Hippocratic Collection as it at present exists. The word is explained in nearly the same sense by Psellus.[2]

δραστικός, *active, efficacious*, applied to medicines in general, and not especially (as in modern times) to purgatives.[3]

δρεπανοειδὲς ὄργανον, *the falciform instrument*, the name of a sort of knife used for fistula in ano.[4]

δρῶπαξ, *a pitch-plaster*, seems to have been considered a comparatively modern term in the time of Galen,[5] in the second century after Christ, though it had been used by writers in the previous century.[6] The more ancient word was πίττωσις or πιττοκοπίη. It was used as a slight counter-irritant, and as a depilatory, and for the singular purpose of causing a slight degree of swelling, and thus creating an appearance of plumpness in parts of the body that were in reality thin or wasted. Thus, Dioscorides speaks of ὁ τῶν σωματεμπόρων δρῶπαξ, "the dropax of the slave-dealers," which expression is not explained by Sprengel, but is illustrated by a passage, where Galen[7] describes the whole process. There is a good note on the word (which has been used in this article) in Dr. Daremberg's Oribasius, tome ii, p. 884. See also Adams's Commentary on Paulus Ægineta, vol. iii, p. 587.

[1] 'De Appell. Part. Corp. Hum.,' p. 42, l. 2, ed. Clinch.

[2] 'Lex Med.,' in Boissonade's 'Anecd. Gr.,' vol. i, p. 239.

[3] Dioscorides, 'Mat. Med.,' i, 18, tom. i, p. 35, l. 5, ed. Sprengel; Leo, 'Consp. Medic.,' iii, 12, in Ermerins, 'Anecd. Med. Gr.,' p. 133, l. ult.; Theophanes Nonnus, 'De Cur. Morb.,' c. 33, tom. i, p. 136, l. 4.

[4] Leo, 'Consp. Medic.,' v, 19, in Ermerins, 'Anecd. Med. Gr.,' p. 183, l. 1.

[5] Galen speaks of what "the *present* Greeks (οἱ νῦν Ἕλλῆνες) call δρῶπαξ" ('De San. Tu.,' vi, 8, tom. vi, p. 416, l, 8).

[6] Archigenes, in Aëtius, i, 3, 180, p. 159, ed. H. Steph.; and in Leo, 'Consp. Medic.,' ii, 15, in Ermerins, 'Anecd. Med. Gr.,' p. 121; Dioscorides, 'Eupor.,' i, 240; ii, 35, vol. ii, p. 221, l. ult.; p. 249, l. 7; Martial, 'Epigr.,' iii, 74, 1; x, 65, 8.

[7] 'Meth. Med.,' xiv, 16, tom. x, p. 998.

δρωπακίζω (Lat., *dropaco*), *to apply a pitch-plaster*, sometimes as a depilatory,[1] sometimes as a counter-irritant[2].

δροπάκινος, *of the nature of a pitch-plaster*.[3]

δρωπακισμός, *the application of a pitch-plaster*,[4] as a counter-irritant.[5]

δρωπακιστός, *of the nature of a pitch-plaster*,[6] used as a counter-irritant for the purpose of producing plumpness. (See Δρῶπαξ.)[7]

δυναμερόν, *a medical formulary*, or *collection of medical receipts*.[8]

δύναμις, *faculty* (Lat., *facultas*), the power of performing any function or action (ἐνέργειαν),[9] in which sense the word ψυχή was sometimes used as synonymous with it.[10] The ancient physiologists for the most part considered that there were *three* faculties concerned in the production and management of an animal, ζῶον, viz. (1) the *natural*, φυσική (called also ἐπιθυμητική, or θρεπτική), connected with the liver and veins, and necessary for the proper nourishment of the body; (2) the *vital*, ζωτική (called also θυμοειδής), connected with the heart and arteries, and necessary for the maintenance of heat in the body; and (3) the *animal*, ψυχική (called also λογική), connected with the brain and nerves, and necessary for the supply of sensation and motion.[11] The *natural faculties*, φυσικαὶ δυνάμεις, common to all parts of the body, but especially observable in the stomach, were considered to be *four*, viz. (1) the *attractive*, ἑλκτική; (2) the *retentive*, καθεκτική; (3) the *assimilative*, ἀλλοιωτική; and (4) the *expulsive*, ἀποκριτική.[12] These points are mentioned in various parts of Galen's works,[13] and especially in his treatise 'De Naturalibus Facultatibus;' and the different uses of the word *faculty* adopted by modern physiologists may be found in Castell's and other lexicons.

The phrases αἱματοποιητικὴ δύναμις,[14] πεπτικὴ δύναμις, and σφυγμικὴ

[1] As in Lucian, 'Demon.,' § 50.

[2] Leo, 'Consp. Medic.,' ii, 15, in Ermerins, 'Anecd. Med. Gr., p. 121.

[3] Alexander Trallianus, viii, 5, p. 420, l. 25, ed. Basil. The text has δρωπάκιον, but this is probably a typographical mistake for δρωπάκινον.

[4] Dioscorides, 'De Venen. Anim.,' c. 3, tom. ii, p. 65, l. pen.; Theophanes Nonnus, 'Curat. Morb.,' c. 210, tom. ii, p. 170, l. antep., ed. Bernard, where the word is written δροπακισμός.

[5] Not in *Dioscorides* as a depilatory. (See Liddell and Scott.)

[6] Galen, 'Comment. in Hippocr. *De Offic. Med.*,' iii, 33, tom. xviii B, p. 894, l. 12.

[7] Not in *Galen* as a depilatory. (See Liddell and Scott.)

[8] Leo, 'Consp. Medic.,' iv, 1, 11, in Ermerins, 'Anecd. Med. Gr.,' pp. 153, 157; Palladius, 'Comment. in Hippocr. *Epid. VI*,' in Dietz, 'Schol. in Hippocr. et Gal.,' vol. ii, p. 98, ll. 20, 22.

[9] Galen says the *faculty*, δύναμις, is the cause of the *function*, ἐνέργεια; the result of which is the *work*, ἔργον ('De Facult. Natur.,' i, 2, tom. ii, p. 6, l. ult.

[10] Id., 'De Meth. Med.,' ix, 10, tom. x, p. 635, ll. 10, 15; p. 636, ll. 3, 4.

[11] Id., ibid., and 'Comment. in Hippocr. *De Alim.*,' iii, 10, tom. xv, p. 292, l. 8, &c.

[12] Id., 'De Facult. Natur.,' iii, 6, tom. ii, p. 160; 'De Usu Part.,' iv, 7, tom. iii, p. 275; Alexander Aphrodis., 'Probl. Phys.,' ii, 60, in Ideler's 'Phys. et Med. Gr. Min.,' vol. i, p. 70.

[13] See Kühn's Index, art. *Facultas*.

[14] Theophilus, 'De Corp. Hum. Fabr.,' ii, 12, p. 78, l. 10, ed. Oxon.; 'De Urin.,' Præf., vol. i, p. 262, l. 9, in Ideler's 'Phys. et Med. Gr. Min.;' 'Comment. in Hippocr. *Aphor.*,' vii, 5, in Dietz, 'Schol. in Hippocr. et Gal.,' vol. ii, p. 521, l. 8.

δύναμις, are also found, applied (respectively) to the veins, the stomach, and the heart; and probably other similar terms are applied to the different functions of other parts of the body.[1]

δύναμις is used not unfrequently for a *drug* or a *medicinal compound*,[2] and in at least one place for a *collection of medicinal formulæ*.[3]

δυσαισθησία, all kinds of *impaired or depraved sensation*,[4] especially, perhaps, *diminished sensation*,[5] as distinguished from ἀναισθησία, *insensibility*;[6] used also in the plural,[7] but not applied exclusively (or perhaps especially) to the sense of touch.

δυσαίσθητος, *having one or more of the senses dulled or impaired*.[8]

δυσαισθητέω, to be δυσαίσθητος,[9] probably applicable to *any* of the senses; in the passage referred to it is the sense of *hearing* that is impaired.

δυσάκεστος (or δυσήκεστος), *hard to be healed*, applied to bed-sores; synonymous with δυσίατος.[10]

δυσαλθής, *hard to be cured* (adj.), applied both to ailments[11] and also to persons.[12]

δυσαλθῶς, *hard to be cured*[13] (adv.).

δυσαλλοίωτος, *hard to be changed*, opposed to εὐαλλοίωτος;[14] applied to articles of food,[15] or to a diathesis,[16] &c.

δυσανάγωγος, *hard to be brought up*, as viscid sputa.[17]

δυσανάδοτος, *hard to be distributed*, as of the chylified and sanguified food throughout the body: not *hard to be digested*, as if synonymous with δύσπεπτος, from which word it is distinguished by Athenæus,[18] and to which it bears the same relation as ἀνάδοσις to πέψις.

[1] Galen, 'De Facult. Natur.,' i, 4, vol. ii, p. 9, ll. 7, 16, 17.

[2] Dioscorides, 'De Venen.,' c. 19, tom. ii, p. 77, l. penult., ed. Sprengel; Aretæus, 'Cur. Chron.,' i, 4, p. 312, l. 1, ed. Kühn; Galen, 'De Compos. Medic. sec. Loc.,' iv, 4, tom. xii, p. 716, l. ult.; 'De Compos. Medic. sec. Gen.,' iii, 2, tom. xiii, p. 593, l. penult.

[3] Oribasius, 'Coll. Medic.,' x, 33, tome ii, p. 450, l. 3, where see Dr. Daremberg's note, p. 893.

[4] Galen, 'De Sympt. Differ.,' c. 2, tom. vii, p. 56, ll. 9, 10.

[5] Soranus, 'De Morb. Mul.,' c. 116, p. 267, l. 13, ed. Dietz; Galen, 'De Anat. Admin.,' iii, i, tom. ii, p. 344, ll. 2, 14; p. 345, l. 1.

[6] Galen, 'De Sympt. Differ.,' c. 2, tom. vii, p. 53, l. 15.

[7] Id., ibid., p. 56, ll. 9, 10.

[8] Alexander Aphrod., 'Med. et Phys. Probl.,' i, 72, in Ideler's 'Phys. et Med. Gr. Min.,' vol. i, p. 23, l. 22.

[9] Alexander Trall., i, 13, p. 45, l. 3, ed. Basil.

[10] Hippocrates, 'De Fract.,' c. 29, tome iii, p. 516, l. 8, ed. Littré; and Galen's 'Comment.,' iii, 26, tom. xviii B, p. 573, l. 6.

[11] Hippocrates, 'De Artic.,' § 41, tome iv, p. 180, l. 10, ed. Littré; Aretæus, 'Caus. Chron.,' i, 14, 16, p. 112, l. 10; p. 120, l. 8, ed. Kühn.

[12] Aretæus, ibid., i, 8, p. 89, l. 10.

[13] Theophanes Nonnus, 'De Morb. Cur.,' cap. 272, vol. ii, p. 328, l. ult., ed. Bernard.

[14] Galen, 'Ars Med.,' cap. 17, tom. i, p. 348, l. 11; Theophilus, 'Comment. in Hippocr. *Aphor.*,' in Dietz, 'Schol. in Hippocr. et Gal.,' vol. ii, p. 309, l. 4.

[15] Pseudo-Hippocr., 'De Alim.,' § 49, tome ix, p. 118, l. 4, ed. Littré.

[16] Galen, 'Adhort. ad Art.,' cap. 11, tom. i, p. 30, l. 1.

[17] Dioscorides, 'Mat. Med.,' i, 1, tom. i, p. 10, l. 11, ed. Sprengel.

[18] 'Deipnos.,' iii, 42, p. 91 E. So also in 'Hippiatr.,' p. 1, l. 25, quoted in H. Stephani 'Thes. Gr.'

δυσανάκλητος, *hard to be recalled*, viz. to health (compare *ἀνάκλησις*, *εὐανάκλητος*). In Dioscorides,[1] *δυσανακλήτως ἔχειν* means *to be hard to be recalled to their senses*, not to their health.

δυσανασκεύαστος, *hard to be restored*, viz. to health.[2]

δυσανάσφαλτος, *one that recovers his health with difficulty*, opposed to *εὐανάσφαλτος*.[3]

δυσάνιος. See *δυσήνιος*.

δυσαποκατάστατος, *hard to be restored*, as a fracture or dislocation.[4]

δυσαπόκριτος, *hardly able to give an answer to a question*, as a person affected with lethargy.[5]

δυσαρεστέω, *to be indisposed*, distinguished by Aëtius[6] from *πυρέττω*, *to have a fever*, inasmuch as a person who is *δυσαρεστῶν* is still able to go about his usual work.

δυσαρέστησις, *indisposition*, *malaise*, rendered by Cælius Aurelianus "corporis displicentia."[7]

δυσδιαφορησία, *a difficulty of dispersing or dissipating*, not necessarily relating to perspiration.[8]

δυσδιαφόρητος, *hard to be dissipated or discussed*,[9] not necessarily relating to perspiration.[10]

δυσδιέγερτος κατα φορά, applied to the deep sleep of lethargy.[11] (See the next word.)

δυσέγερτος, *hardly able to be roused*, as a person affected with lethargy;[12] *δυσέγερτος ὕπνος*, applied to the deep sleep of coma;[13] *τὸ δυσέγερτον*, used as a substantive, the quality *of being hard to rouse*.[14]

δυσεκθέρμαντος (adj.),[15] *δυσεκθερμάντως*[16] (adv.), *hard to be warmed*.

δυσεκπύητος, *hard to be brought to suppuration*, as a tumour.[17]

[1] 'De Venen.,' cap. 16, tom. ii, p. 28, l. 5, ed. Sprengel.

[2] Alexander Trall., xii, cap. ult., p. 776, l. 33, ed. Basil.

[3] Pseudo-Hippocrates, 'De Alim.,' § 28, tome ix, p. 108, l. 12, ed. Littré.

[4] Pseudo-Galen, 'Introd.,' cap. ult., tom. xiv, p. 792, ll. 4, 7; p. 796, l. ult.

[5] Paulus Ægin., iii, 9, p. 28 B, l. 7, ed. Ald.

[6] v, 5, or ii, 1, 5, p. 191, ed. H. Steph.

[7] 'Morb. Chron.,' iii, 6; v, 10, 11, pp. 463, 583, 600.

[8] Cassius, 'Problem.,' § 66, in Ideler's 'Med. et Phys. Gr. Min.,' vol. i, p. 163, l. 31.

[9] Paulus Ægin., iv, 18, p. 63 B, l. 37; Theophanes Nonnus, 'Cur. Morb.,' c. 242, vol. ii, p. 244, l. 2, ed. Bernard.

[10] Galen, 'De Alim. Facult.,' i, 23, tom. vi, p. 536, l. 11; Stephanus Athen., 'Comment. in Gal. *Therap. ad Glauc.*,' in Dietz, 'Schol. in Hipp. et Gal.,' vol. i, p. 274, l. 2; p. 285, l. 13, p. 300, l. ult.

[11] Galen, 'Defin. Med.,' c. 235, tom. xix, p. 413, l. 5.

[12] Paulus Ægin., iii, 9, p. 28 B, l. 7, ed. Ald.

[13] Theophanes Nonnus, 'Cur. Morb.,' c. 29, vol. i, p. 114, l. 1, ed. Bernard.

[14] Theophilus, 'Comment. in Hippocr. *Aphor.*,' ii, 3, in Dietz, 'Schol. in Hippocr. et Gal.,' vol. ii, p. 297, ll. 20, 21.

[15] Antyllus, in Oribasius, 'Coll. Med.,' x, 13, tome ii, p. 413, l. 1, ed. Daremb.; Palladius, 'De Febr.,' cap. 19, p. 66, l. pen., ed. Bernard.

[16] Antyllus, ibid., cap. 29, p. 451, l. 4.

[17] Paulus Ægin., iv, 18, p. 63 B, l. 37; Theophanes Nonnus, 'Cur. Morb.,' c. 242, vol. ii, p. 244, l. 1.

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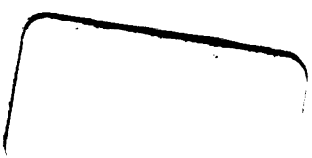
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